MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. LXIX. NOVEMBER 13, 1908. NO. 1.

H. F. NEWALL, Esq., M.A., F.R.S., PRESIDENT, in the Chair.

Warin Foster Bushell, The Hermitage, Harrow;
Charles P. Butler, A.R.C.Sc.. F.R.P.S., Solar Physics Observatory, South Kensington, S.W.;
William Doberck, Ph.D., M.R.I.A., Kowloon, Elgin Road, Sutton, Surrey;
James Nangle, Technical College, Sydney, New South Wales, Australia, and Private Observatory, Tupper Street, Stanmore, near Sydney;
Charles W. Raffety, Wynnstay, Woodcote Valley Road, Purley, Surrey;
Rev. T. J. Williams-Fisher, M.A., Rector of Norton, Atherstone,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Samuel Butler, The Cedars, Compton, Wolverhampton (proposed by E. B. Knobel);
Charles Everitt, 67 Boston Avenue, Southend-on-Sea (proposed by Sir D. Gill);

J. William Gifford, Oaklands, Chard, Somerset (proposed by Sir D. Gill);

James Thomas Jackson, M.A., Assoc.M.Inst.C.E., Engineering School, Trinity College, Dublin (proposed by E. T. Whittaker);

Dr. Arthur Scott King, Superintendent of the Physical Laboratory, Mount Wilson Observatory, California, U.S.A. (proposed by George E. Hale);

Robert Owen, Merchant, 25 Mostyn Street, Llandudno, North Wales (proposed by Rev. J. Cairns Mitchell); and

Dr. Charles Edward St. John, Mount Wilson Observatory, California, U.S.A. (proposed by George E. Hale).

One hundred and sixty-three presents were announced as having been received since the last meeting, including, amongst others:—

E. W. Brown, Inequalities in the motion of the Moon due to the direct action of the planets (Adams Prize Essay), presented by the author; J. J. Thomson, Treatise on the motion of Vortex Rings (Adams Prize Essay), presented by Prof. Turner; Reports of the Geodetic Survey of the Transvaal and Orange River Colony, presented by the Transvaal and Orange River Governments; Catalog der Astronomischen Gesellschaft, Washington zone, -14° to -18°, presented by the Astronomische Gesellschaft; Helsingsfors, Catalogue Photographique, coordonnées rectilignes, tome 3, presented by the Helsingfors Observatory.

Astrographic Chart: 60 charts, presented by the French Minister of Public Instruction; 26 charts, presented by the Tacubaya Observatory; 84 charts, presented by the Royal Observatory, Greenwich.

Photographs of Comet *c* 1908:—series of 127 transparencies, presented by the Astronomer Royal; 14 lantern slides, presented by Prof. Barnard; 19 transparencies, presented by Mr. Percy Morris; 4 prints, presented by Mr. R. C. Johnson.

LUNAR NOMENCLATURE.

1. On p. 134 of vol. lxviii. of the *Monthly Notices* there was printed, by the courtesy of the Editors, a circular outlining several alternative proposals for lunar nomenclature. In order to enable the Committee appointed by the International Association of Academies to judge of the reception which such proposals, if definitely put forward, were likely to meet with, four questions were suggested for the consideration of those sufficiently interested. A number of kind replies have been received, and it seems desirable to publish the results for the general information of astronomers.

The replies will first be given in a tabulated form, and then such remarks as cannot be included in the table will be appended.

The opinions of members of the Committee have not been included. Some of them have been provisionally indicated in the circular on p. 134; but they are naturally subject to modification, in the light of information received and of discussion

The four questions suggested for definite replies were:—

(1) Do you approve the proposal of some mechanical or numerical system of nomenclature *in place of* the old?

(2) Do you approve the proposal of some such system *in addition to* the old?

(3) If such a system is to be adopted, which of the three proposed below seems, on the whole, preferable?

(4) Are your comments and replies intended solely for the Committee, or may they be published if thought desirable?

When this last question has not been expressly answered, initials only are given below: the word 'yes' in column 4 means that permission to publish is expressly given.

So far as they can be definitely tabulated, the answers were as follows:—

Name.	1.	2.	3.	4
T. W. B.	...	...	Puiseux.	
M. A. B.	No.	No.		
W. L. Elkin.	No.	No.	Puiseux.	Yes.
P. F.	No.	No.		
M. E. J. Gheury.	No.	Yes.	Puiseux.	Yes.
W. Goodacre.	No.	No.	...	Yes.
J. A. H.	No.	No.		
F. H.	...	...	Puiseux.	
P. B. Molesworth.	No.	No.	...	Yes.
M. Moye.	No.	No.	...	Yes.
A. Noël Neate.	No.	No.	...	Yes.
C. D. Perrine.	No.	Yes.	Puiseux.	Yes.
E. C. Pickering.	Yes.	No.	W. H. Pickering.	Yes.
H. G. Tomkins.	No.	...	Puiseux.	Yes.
L. Weinek.	No.	No.	...	Yes.
H. C. W.	No.	No.		
Max. Wolf.	No.	No.	...	Yes.

Additional remarks may be briefly summarised as below :—

T. W. B. :—That part of Professor Pickering's suggestion commends itself in which he proposes to have photographs in different phases for identification of the features.

M. A. B. : — Symbols are not true *names*. . . . Likes Mr. Saunder's proposals better in all respects than the others. . . . There is some force in Dr. Franz's objection to the insertion of long names in a map ; but this difficulty might be got over by using abbreviations or numbers, and giving the full name corresponding to them in the margin, or on an opposite page. . . . If the present system of letters for secondary formations be retained, it seems a necessity that the maps should be divided into regions, either by faint dotted red lines, or in some other unequivocal way. . . . In the case of the stars, the present system did not supersede the old, because there was no old system to be superseded.

W. L. Elkin :—M. Puiseux's system seems open to the fewest objections ; but Mr. Saunder's proposal simply to designate formations by their selenographical co-ordinates seems far preferable. Possibly the Harvard plan of designating negative values by italics or underlining (*e.g. 24, 72* for -24° long. and -72° lat.) would be advisable.

M. E. J. Gheury :—Anyone who has used an atlas, such as the Paris photographic Atlas of the Moon, and has appreciated the ease with which descriptions can be followed by means of rectangular co-ordinates, will at once recognise the advantage of such a system over any other. . . . Dr. Franz's proposals may lead to some incoherent sentences unless a different type is used ; such a sentence as this might occur : "Some time ago Ago was found brighter simultaneously with Age. . . ." Notes with special pleasure Professor Pickering's recommendation that it should be possible to buy photographs or maps of a single region.

W. Goodacre :—(1) Continue the present names, or substitute others in place of some of them.

(2) Omit double names, and those with a prefix or suffix.

(3) Retain the present names for the seas and mountain ranges.

(4) Divide the disk into areas, each of which would be associated with some dominant formation, the bounding lines of each area to be those suggested by the contour of the surface.

(*a*) The larger crater rings in each area (say of not less than 10 miles in diameter) to be lettered by using Roman capitals.

(*b*) The smaller craters to be numbered, and small Roman letters to be used for other details such as spots, hills, or depressions.

(*c*) Clefts and valleys to be marked by small Greek letters.

J. A. H. :—Would like to see the present names retained. . . . An official list would, of course, have to be drawn up, and this naturally entails a map on which to identify them ; photographs

would not be so efficient as a well-drawn outline map. Speaks here from experience gained in using the Belgian reproductions of the Paris atlas. Small formations are often lost in shadow in just those photographs that show the rest of the region best: if it is attempted to give both illuminations (indeed *three* illuminations, as Professor Pickering wisely demands some plates taken under a high sun), the number of plates required will become excessive.

P. B. Molesworth:—Approves Mr. Saunder's proposals. Has only one amendment to suggest, and that refers to the nomenclature of the 'rills.' Would prefer to see these referred to (like other minor formations) by the name of the principal formation followed by a figure (*e.g.* Triesnecker 4, Gassendi 4). Would in every case give the rills in Schmidt's Catalogue precedence, and would, as far as possible, give them the same consecutive order as in his catalogue.

M. Moye:—We must not drop the old system; we must be content with a new system for minor formations—craterlets, unnamed spots, etc. Prefers Mr. Saunder's proposals.

A. Noël Neate:—Thinks the map should be drawn to a scale of not less than 100 inches to the diameter, and represent the Moon under mean libration.

C. D. Perrine:—Prefers Puiseux's proposals, except that he would start at the meridian and equator and letter *both ways*. Approves atlas as suggested by Professor W. H. Pickering; but suggests printing the plates by heliogravure, though more expensive.

E. C. Pickering:—The question of Lunar Nomenclature is here presented to me for the first time. We have, however, at this Observatory, had occasion to use, frequently, special nomenclatures for faint stars, variables, and other similar objects.

My replies to your questions are as follows:—

(1) Yes. That is, the new system should be independent of the old, although it would be often convenient to use both names. Thus, the variable star T Andromedæ is $+26^{\circ}43$, but we find that the designation, 001726, is a very convenient one. See *H.A.*, **55**, 7.

(2) No. See 1.

(3) That proposed by my brother, Professor W. H. Pickering, with certain modifications. Figures are preferable to letters, especially in printing tables, since all the type have the same width. The type for the letters m and w are much wider than those for f, i, and l. The designations of points on a photograph by their co-ordinates have frequently been found very convenient, in fact almost indispensable, in the *Harvard Annals*. See *H.A.*, **26**, 220, **51**, 4, also *H.C.*, 71. The use of the telephone and the numbers of houses in large cities using numbered streets have accustomed the public to memorise groups of five and even six figures. Thus, eighty-nine one hundred and first street is as

easily remembered as b t f k w. It seems to me, therefore, that the nomenclature used in *H.A.*, 51, would be convenient for faint objects, supplementing the usual notation for bright objects, as we use Durchmusterung numbers for the faint, and Bayer letters for the bright stars. The method of co-ordinates has the great advantage that it can be extended indefinitely to smaller and smaller objects. This will be indispensable with improved instruments and photographs. An artificial notation requires that each designation shall be explained before it can be used; a very serious objection. It seems unnecessary to go to the expense of publishing another Atlas of the Moon, arranged according to a systematic plan, since one already exists in *H.A.*, 51, and the plates are at the service of the Committee if desired for this purpose.

(4) My comments and replies may be published if the Committee desires to do so. I hope that this may be done in the case of other astronomers, as otherwise the material collected will have but little value.—Yours very truly,

EDWARD C. PICKERING.

H. G. Tomkins:—Would retain the present system, but suggests the division of the surface into districts, each bearing the name of some prominent formation in it. Boundaries to be lines of latitude and longitude, 20° apart. Positions of objects in each division to be recorded. Gives a list of 22 divisions. According to the system,

Eratosthenes would be Copernicus 28/38.
Linné would be Posidonius 61/61.

Thinks map very desirable.

L. Weinek: — Prefers Mr. Saunder's suggestions, *i.e.* to rectify Mädler's nomenclature. Latin letters for depressions and Greek letters for elevations. Calls attention to Mädler's proposals on p. 28 of *Der Mond*, which would help in clearing up doubtful points. Suggests the construction of new charts for the foreshortened portions near the limb, as though they were seen from their zenith.

H. C. W.:—Is opposed to any great change. Would adhere to the names, letters, and numbers of Neison as far as possible, since Neison's book is probably in the possession of five times as many workers as any other large map such as Schmidt's.

Dr. Max. Wolf:—In spite of the ingenuity of the other proposals, can assent only to Mr. Saunder's,—

(1) Because of publications already in existence, which would have to be translated.

(2) Because a numerical system is not so convenient for amateurs.

(3) Because the system of actual co-ordinates must be in use

ultimately in addition to the names; and any artificial equivalent for co-ordinates would be a *third* system in use.

The decisions of the Committee will be taken in the light of these opinions, for which we are grateful to the writers. At present we are still without a chairman, though the appointment is under consideration by the Accademia dei Lincei, as leading academy for the time being.

H. H. Turner,
Secretary to the Committee on Lunar Nomenclature.

University Observatory, Oxford:
1908 *October* 26.

Comet Committee.

The following communication has been received from Professor Barnard:—

"The Astronomical and Astrophysical Society of America, at its meeting last August, appointed a committee on comets, consisting of—

George C. Comstock, *Chairman.*
Edward E. Barnard.
Charles D. Perrine.
Edward C. Pickering.

"The committee proposes to assist in making as effective as possible, co-operation in the physical study of comets, and especially with respect to the coming apparition of Halley's comet."

Note on the Regnal Years in the Aramaic Papyri from Assuan.
By E. B. Knobel.

In my paper on these papyri published in the *Monthly Notices* for March 1908, I referred to the regnal years of the kings of Persia, but without attempting any explanation as to how those years were reckoned. It is important to investigate the question, as the calendar dates must necessarily depend upon the determination of the accession of the kings, whence the commencement of the regnal years can be fixed.

Regnal years at this period appear to have been reckoned in three different ways. First, they were determined from the accession of the king precisely as the regnal years in this country: Oppert states that this system was used in Assyria, and was that adopted in the Bible; secondly, they were reckoned as beginning on the 1st. Nisan following the accession; and thirdly, the regnal years were considered to commence with the 1st. Thoth of Nabonassarean and Egyptian years preceding the actual accession. This is the system adopted in Ptolemy's canon.

The records which enable us to determine the dates of accession of the kings from Xerxes the Great to Darius Nothus are fairly clear.

Xerxes the Great.

Fynes Clinton (*Fasti Hellenici*) states that the accession of Xerxes was about the spring of B.C. 485. Oppert, however, has called attention to a Babylonian tablet which records that Darius Hystaspes, the father of Xerxes, was living September B.C. 485, and he concludes definitely that the accession of Xerxes was in the autumn of that year.* I think this may be accepted as the true period of his accession.

Artabanus.

Xerxes was assassinated by Artabanus in the beginning of the archonship of Lysitheus—the 4th. year of the 78th. Olympiad. The commencement of this archonship is well determined as July B.C. 465, consequently the accession of Artabanus can be fixed as July or August B.C. 465.

Artaxerxes Longimanus.

Artabanus reigned for seven months, on which all authorities agree, which brings us to February B.C. 464, when he was killed by Artaxerxes, whose accession is thus fixed with considerable accuracy.†

* "La fixation exacte de la chronologie des derniers rois de Babylone."

† Thucydides records that in the 4th. year of the 78th. Olympiad, July B.C. 465 to June B.C. 464, Themistocles went up the country with one of the Persians who dwelt on the coast, and sent a letter to Artaxerxes, the son of Xerxes, who had just succeeded to the throne. This journey would probably be made in the winter, and not in the summer.

Xerxes II. and Sogdianus.

The death of Artaxerxes is recorded by Thucydides as occurring in the winter of the archonship of Stratocles—the 4th. year of the 88th. Olympiad—about December B.C. 425 or January B.C. 424—and the greater part of the year B.C. 424 is occupied by the short reigns of Xerxes II. and Sogdianus.

Darius Nothus.

Fynes Clinton says that the accession of Darius Nothus may be gathered from Thucydides, who, in discussing the third treaty in the 20th. year of the Peloponnesian war, places the 13th. year current of his reign in the winter of the archon Callias, in the 1st. year of the 92nd. Olympiad, about February B.C. 411. This testimony critically agrees with the date of Ptolemy's canon, which places the 1st. Thoth of Darius Nothus at December 7 B.C. 424, precisely 12 years 2 months before the date of the treaty in Thucydides. This is confirmed by Diodorus, who places the accession about the 6th. month of the archon Isarchus, the 1st. year of the 89th. Olympiad, December B.C. 424.

Accession Dates.

Xerxes the Great . .	September or October	B.C. 485.
Artaxerxes Longimanus .	February	,, 464.
Darius Nothus . . .	December	,, 424.

We have now to consider the regnal years given in the papyri, and determine the system which best accords with those dates. (With regard to J, I have come to the conclusion, from a careful examination of the original negative, that the second year of the king therein given must be read as 9, and not 8.)

It seems to be established that the short reigns of the above kings, Artabanus, Xerxes II., and Sogdianus, are included in the regnal years of the predecessor, or distributed between the last year of the predecessor and the first year of the successor.

Thus	21	regnal years are ascribed to	Xerxes the Great.
	41	,, ,,	Artaxerxes.
	19	,, ,,	Darius Nothus.

Regnal years as given in the papyri :—

A. 15th. year of Xerxes.
B. 21st. year (of Xerxes), the beginning of the reign when Artaxerxes the king ascended the throne.
C and D. 6th. year of Artaxerxes.
E. 19th. ,, ,,
F. 25th. ,, ,,
G. ?
H. 4th. year of Darius.
J. 8th. and 9th. year of Darius.
K. 13th. and 14th. year of Darius.

If we assume that the regnal years were reckoned from the accession, then the 15th. year of Xerxes would not include September 12 B.C. 471, and the 21st. year would not commence till 3 or 4 months after he was dead — clearly this method must be inapplicable.

If we adopt the view of Oppert that the regnal years began with 1st. Nisan after the accession, then September 12 in the 15th. year of Xerxes would be B.C. 470, and the 21st. year would begin nine months after his death and two months after the accession of Artaxerxes. Certainly this could not have been the method adopted in the papyri.

But if the regnal years began with the 1st. Thoth preceding the accession, then—

A. Sept. 12 B.C. 471 was in the 15th. year of Xerxes.
B. The 21st. year of Xerxes would begin Dec. B.C. 466, or seven months before his death.

C. and D.	Dec. 11	B.C.	459	was in the	6th.	year of Artaxerxes.
E.	Nov. 17	,,	446	,,	19th.	,, ,,
F.	Aug. 26	,,	440	,,	25th.	,, ,,
H.	Sept.	,,	420	,,	4th.	,, Darius.
J.	Dec. 16	,,	416	,,	9th.	,, ,,
K.	Feb. 10	,,	410	,,	14th.	,, ,,

With the exception of the difficult case of D, which in my former paper was guessed at, the whole of the above agree with the years I have ascribed. I do not think there can be much doubt that the regnal years in question were reckoned from the 1st. Thoth preceding the actual accession.

The view of the late M. Oppert that the regnal years in the Bible were reckoned from the accession is not correct in one notable instance. The first verse of the first chapter of Nehemiah states, "And it came to pass in the month Chisleu, in the twentieth year, as I was in Shushan the palace": * and the second chapter, referring to subsequent proceedings, begins, "And it came to pass in the month Nisan, in the twentieth year of Artaxerxes the king." Here we have Chisleu (November or December) preceding Nisan (March and April) in the *same* regnal year. Clearly the regnal year did not begin with Nisan; and if it began with the accession of Artaxerxes (February), then Nisan would have preceded Chisleu; but if the year began with Thoth, the explanation is complete. Thus the date of the first chapter of Nehemiah is December B.C. 446, and that of the second March or April B.C. 445.

We are now in a better position to consider the somewhat anomalous statement in papyrus B, with the explanations suggested by Mr. Fotheringham and Dr. Schürer.

On the 18th. of Chisleu that is the (7th.) day of Thoth, the 21st. year (of Xerxes), the beginning of the reign when Artaxerxes the king ascended the throne."

* Shushan was the winter palace of the kings of Persia.

The papyrus is broken at one date, and only four strokes of the number expressing the day of Thoth are left. Mr. Cowley completes this, and considers it to be the number 7. The reign of Artabanus occupied the latter part of the 21st. regnal year of Xerxes and the commencement of the 1st. regnal year of Artaxerxes. On account of the intervention of the reign of Artabanus, it is impossible that any portion of the 21st. year of Xerxes could embrace any of the 1st. year of Artaxerxes, and it is noticeable that the papyrus does not here give any regnal year of Artaxerxes. I was in error in the former paper in departing from the text and designating B as the 1st. regnal year of Artaxerxes.

Mr. Fotheringham and Dr. Schürer suggest that the date missing in the papyrus is 17th. Thoth, January 3 B.C. 464. Mr. Cowley is of opinion that there is not space in the gap for the number 17. But supposing there were sufficient room for the number 17, then January 3 B.C. 464 would be during the reign of Artabanus, and the colony at Syene could never have heard of Artaxerxes, who did not ascend the throne till the following month. Moreover, it must be pointed out that Babylon is more than 1300 miles' journey from Syene, and therefore the news of any change in dynasty at the capital would not reach Upper Egypt for several months. The date I have ascribed to B is ten months after the accession of Artaxerxes, and I do not think the facts of the case are inconsistent with that conclusion.

D. It is natural that the suggestion I hazarded about the date of this papyrus should be received with considerable scepticism, though the same idea had occurred to others.

J. As I have pointed out above, this should be the 9th. year of Darius, and I agree with Mr. Fotheringham and Dr. Schürer in their remarks on J and K, that the lower regnal year is computed from Nisan and the higher from Thoth.

The problem suggested by these papyri has, I think, to be considered more from a practical point of view, for it does not admit of exactitude. There is a colony of Jews, somewhat isolated and far removed from Jerusalem and Babylon, and for their needs they would have to adopt some form of regular calendar. What course would they probably adopt? I have considered that what was current in Babylon during the captivity would be continued by the Jews in their subsequent migration ; and the direct connection between the colony at Syene and Babylon leads me to consider it not unwarrantable that the calendar months at Babylon should have been adopted by them. The dates I have given hang together on a definite system, which is not inconsistent with a calendar constructed on a mean moon, as suggested by Mr. Fotheringham.

Calendar Dates in the Aramaic Papyri from Assuan.

By J. K. Fotheringham.

(*Communicated by E. B. Knobel.*)

Those who are interested in ancient calendars and their astronomical significance will be grateful to Mr. Knobel for the close examination that he has given the dates in the Assuan papyri in the *Monthly Notices* of March 1908. Mr. Knobel's verification of these dates is in the majority of cases beyond controversy, and is a marked improvement on the dates given by Mr. Cowley from a mere reckoning by the years of Persian kings without reference to astronomical data. There are, however, two instances where it seems to me that Mr. Knobel's dates are capable of emendation, and I think it is also doubtful whether he is right in the calendar principles by which he attempts to explain them.

The papyri edited by Professor Sayce and Mr. Cowley * belong to a series of Aramaic papyri, which also includes three papyri edited by Professor Sachau † and translated into English by Canon Driver, ‡ and one papyrus edited by Professor Euting.§ All these papyri contain lunar dates with Aramaic month-names, but in those edited by Professor Sayce and Mr. Cowley these dates are accompanied by the corresponding dates of the Egyptian calendar, doubtless because they are all of the nature of contracts dealing with rights of property in Egypt, whereas the papyri edited by Professor Sachau and Professor Euting, which are of the nature of petitions to Persian authorities outside Egypt, contain none but the Aramaic month-names.

It has been assumed by all writers whose works have met my eye that the months with Aramaic names belong to the Jewish calendar, probably because the papyri belonged to a Jewish community. The argument does not appear to me to be conclusive. It is well known that these names are of Babylonian origin, and were not adopted by the Jews till the captivity, nor were they adopted by the Jews only, but also by the other peoples of Syria and Mesopotamia. ‖ It may therefore be better to call these month-names Aramaic until it is determined to what calendar they belong.

A very brief inspection of the papyri will show that these Aramaic dates belong to a lunar calendar; and since the Egyptian calendar is well known, each year consisting of 365 days, it should be possible by a comparison of a table of Egyptian dates with a table of new moons to date precisely each papyrus that bears a double date, and to fix accurately the regnal years of Persian kings to which they are referred. The papyri that bear only an Aramaic

* *Aramaic papyri discovered at Assuan*, 1906.

† *Abhandlungen der königl. preuss. Akademie der Wissenschaften*, 1907.

‡ *The Guardian*, Nov. 6, 1907, p. 1827 f.

§ *Notice sur un papyrus Égypto-Araméen de la Bibliothèque impériale de Strasbourg*, 1903.

‖ See Schiaparelli, *Astronomy in the Old Testament*, Oxford, 1905, p. 111.

date cannot by themselves be dated with the same precision, but as they too are assigned to definite regnal years, the other papyri do in effect enable us to date them also.

In Mr. Knobel's citations of the text of the papyri and his interpretation there is little which calls for criticism. I have examined each date in detail, and am inclined to accept Professor Schürer's conclusion in almost every case.* Professor Schürer and Mr. Knobel are, as will be seen, certainly right in accepting the higher numerals, bracketed by Mr. Cowley, as the only ones capable of bringing the chronology into any consistency. In papyrus B, where Mr. Knobel accepts Mr. Cowley's conjectural restoration of a lacuna with the date 6th. (7th.?) of Thoth, Professor Schürer prefers to read 17th. Again in papyrus J, where Mr. Knobel, following Mr. Cowley, reads 7th. (8th.?) for the year of Darius according to the Egyptian reckoning, Professor Schürer reads 9th. and Mr. Knobel has informed me that he now accepts this reading. In this case Mr. Cowley acknowledged that there seemed to be traces of an additional stroke, but preferred the reading 7th. (8th.?) in order to make the numeral agree with that in the Aramaic reckoning, not realising that the double insertion of the regnal year was due here, as in the following papyrus, to a difference between the Aramaic and the Egyptian reckoning.

To examine the dates more closely, we need, as I have suggested, a comparative table of the Julian and Egyptian calendars, such as is provided by Professor Mahler,† and also a table of new moons, such as is provided by Professor Ginzel.‡ Professor Ginzel gives the new moons in decimals of a day, reckoned from Greenwich mean noon. I have converted these into hours and minutes, reckoned from Assuan midnight. The addition of nine minutes more will convert these dates into Jerusalem time. Professor Ginzel's calculations are based upon Oppolzer's values for lunar and solar constants, and are made by means of Dr. Schram's Mondtafel.§ The method of calculation is far from exact, and the error may easily amount to the greater part of an hour. We have also to allow for possible errors in Oppolzer's values for the constants. By substituting Professor Newcomb's values‖ for Oppolzer's we obtain a date three minutes later for the mean new moon of Elul, 471 B.C., and by substituting Mr. Cowell's values¶ we obtain a date thirty minutes later than Oppolzer's for the same mean new moon. On the other hand, by substituting

* See his article in *Theologische Literaturzeitung*, Feb. 2, 1907. In one case I propose a correction of two days, and in one case I date a papyrus which he leaves undated. Otherwise my dates are the same as his.

† *Chronologische Vergleichungstabellen*—I. *Ägypt etc. griech.* 1888.

‡ *Handbuch der Chronologie* (1906), I, 551–3.

§ *Denkschriften der kaiserlichen Akademie der Wissenschaften Math.-naturw. Klasse*, xlv. (Vienna, 1882), reprinted in Schram's *Kalendariographische und Chronologische Tafeln*, 1908, pp. 356–9.

‖ I take these from Mr. Cowell's paper in *Monthly Notices*, lxv. (1905), p. 863.

¶ *Monthly Notices*, lxvi. (1906), p. 525.

Professor Ginzel's own values, we obtain a date twenty-three minutes earlier than Oppolzer's. For the mean new moon of Shebat, 410 B.C., these differences must be reduced to two minutes, twenty-eight minutes, and twenty minutes respectively. For the intervening new moons the corrections resulting from the substitution of these values will fall between the extremes just given. Mr. Knobel's lunar cycle must, I am afraid, be set aside, partly because we do not know that the calendar with which we are dealing is Jewish, partly because we have no accurate information about the Jewish calendar in the fifth century B.C., and partly because the initial date from which his supposed Jewish calendar is calculated, the eclipse of 14 Tammuz, 523 B.C., really belongs not to the Jewish, but to the Babylonian calendar. We must be content to assume in each case that the lunar month began near the new moon, and see what results from this. Taking the papyrus dates one by one, we get the following results:—

A. 17 (18) Elul = 27 (28) Pachons in 14 (15) Xerxes. The only date that could possibly correspond to 14 (15) Xerxes in which either the 27th. or 28th. of Pachons could be the 17th. or 18th. day of a lunar month is 471 B.C., when 27 (28) Pachons was the Julian 11 (12) September; so that we have 17 (18) Elul = 11 (12) September 471, 1 Elul = 26 August 471. Professor Ginzel gives for the new moon August $24^{d}\ 18^{h}\ 45^{m}$; if this is later than sunset, Elul would appear to have begun at the sunset after new moon. We also get 14 (15) Xerxes = 472–1 or 471–0.

B. 18 Chisleu (18 appears to be the correct figure) = 6 (7) [17?] Thoth in Xerxes 20 (21), beginning of Artaxerxes. Now if Xerxes 14 (15) is 472–1, Xerxes 20 (21), should be 466–5. 6 (7) Thoth would then be 23 (24) Dec. 466, and 17 Thoth would be 3 Jan. 465, impossible dates for the 18th of a lunar month, and exceedingly early for the accession of Artaxerxes. But if Xerxes 14 (15) is 471–0, Xerxes 20 (21) should be 465–4, the year beginning somewhere before Elul, presumably in Nisan, and 17 Thoth will be 2 Jan. 464. Mr. Knobel proposes to identify 6 (7) Thoth with 22 (23) Dec. 464, but Xerxes 20 (21) cannot be extended so late unless we suppose, firstly, that Xerxes 14 (15) is an error for Xerxes 13 (14), and, secondly, that the accession year of Artaxerxes is contrary to the Assyrian and Babylonian method of reckoning, the same as his "first year" and also the same as the 21st. year, the last regnal year, of his predecessor. These assumptions are, I think, too violent to be maintained. We are therefore compelled with Professor Schürer, to adopt the synchronism: 18 Chisleu = 17 Thoth = 2 Jan. 464. It will be observed that as 21 is always given as the last regnal year of Xerxes, the lower numbers, where Mr. Cowley gives us alternatives, already appear highly improbable; for if we were to accept them, the 20th. year of Xerxes would be the accession year of Artaxerxes. We now have 1 Chisleu = 16 Dec. 465, Chisleu beginning at the sunset after the new moon of Dec. $15^{d}\ 1^{h}\ 0^{m}$.

C is too much injured for the dates to be deciphered, but Mr.

Cowley appears to be right in suggesting that it is of the same date as D.

D. 21 Chisleu = 1 Mesore in 6 Artaxerxes. Mr. Knobel explains this on the bold assumption that 1 Mesore ought to be read 31 Mesore, and that 31 Mesore is a name, found nowhere else, for the first Epagomene. He also assumes that 6 Artaxerxes is at latest 460–459, four years later than the year which he identifies as the accession year of Artaxerxes, and, as will be seen hereafter, fourteen years earlier than 19 Artaxerxes. If he had, with Professor Schürer, chosen a date only one lunar month earlier, the most difficult of these assumptions would have been obviated. We then have 21 Chisleu = 1 Mesore = 11 Nov. 460. 1 Chisleu is then = 22 Oct. 460, and Chisleu begins at the sunset following the new moon of Oct. $21^{d}\ 2^{h}\ 12^{m}$. The 6th. year of Artaxerxes is, however, still 460–459, five years after his accession year, but 14 years before what we shall find enumerated as his 19th. year. On either Mr. Knobel's or Professor Schürer's assumption, it appears to be necessary to correct the 6th. year to the 5th., but Professor Schürer's hypothesis involves no further difficulty, and may be accepted as correct.

E. 3 Chisleu = 10 Mesore in 19 Artaxerxes. Both Professor Schürer and Mr. Knobel identify this with 17 Nov. 446. This would give us for 1 Chisleu 15 Nov. 446, and Chisleu would begin at the second sunset before the new moon of Nov. $16^{d}\ 6^{h}\ 2^{m}$, a surprising result, which we should nevertheless be compelled to accept were there not other evidence, to be mentioned hereafter, pointing to an error in this date. For 19 Artaxerxes we get 446–5, agreeing with an accession year of 465–4, but not with a 6th. year of 460–59.

F. 13 (14) Ab = 19 Pachons in 25 Artaxerxes. This is indisputably = 26th August 440. 1 Ab is therefore 14 (13) August. The date of the new moon is given by Professor Ginzel as Aug. $12^{d}\ 19^{h}\ 28^{m}$, so that if we accept the reading 14 Ab, as seems to follow from the regnal years of Xerxes above, Ab would appear to begin at a sunset almost simultaneous with new moon, if anything slightly preceding it. The difference between the two is apparently within the range of error of Professor Ginzel's tables, of ancient Babylonian computations, and even of modern theory. 25 Artaxerxes is clearly 440–39.

G. 26 Tishri = 6 Epiphi. The number of the year is lost here, but Mr. Cowley argues that it cannot be earlier than 446 or later than 440. He himself prefers 440. As his dates for papyri E and F are confirmed by our astronomical investigations, we may accept these dates as they stand. Professor Schürer abandons the attempt to date this papyrus. Mr. Knobel proposes 14 Oct. 446. The only dates astronomically possible appear to be 14 Oct. 446 and 13 Oct. 443. The former gives for 1 Tishri 19 Sept. 446, the month beginning at the sunset after the new moon of Sept. $17^{d}\ 18^{h}\ 31^{m}$, and the latter gives 18 Sept. 443, the month beginning at the fourth sunset after the new moon of Sept. $13^{d}\ 22^{h}\ 48^{m}$.

As nearly all the dates in the series involve a commencement of the lunar month at the sunset immediately following the new moon, I prefer to accept Mr. Knobel's date, in spite of a further difficulty which it involves. We now have in 446 the two dates 26 Tishri and 3 Chisleu separated by only 34 days, but even if we suppose that in this year both Tishri and Marheshvan were 29-day months the interval ought to be 35. If either had 30 days, as was probably the case, the interval should be 36. Now, Chisleu appears in D to begin two days too soon, on the second evening before the new moon instead of on the evening after the new moon. There are therefore two independent reasons for assuming an error of two days in the date of E, and for correcting either 3 Chisleu to 1 Chisleu or 10 Mesore to 12 Mesore. It will be observed that this error is assumed as much by Mr. Knobel's theory as by my own, in spite of Mr. Knobel's protestation against assuming any error in the dates contained in these papyri. It may be remarked that errors in other parts of the papyri are not uncommon.

H. Elul = Payni in 3 (4) Darius. Payni, as Mr. Knobel points out, would in 420, which is the most probable date, run from 2 September to 1 October, and is almost conterminous with a lunar month. This does not permit us to fix the beginning of the month with certainty, but renders September 29 the probable date for the new moon of Tishri.

J. 3 Chisleu, 7 (8) Darius = 11 (12) Thoth, 7 (8) [9?] Darius. If, as Professor Schürer suggests, and as seems probable, 9 is the correct reading in the Egyptian date, we have the regnal year repeated, because it was different in the two calendars used. The date is clearly 15 (16) Dec. 416, so that 1 Chisleu is 13 (14) Dec. If we accept the higher figure, as seems to be indicated by the date in B, and as will be seen by the date in K, Chisleu will begin at the sunset following the new moon of Dec. $12^{d}\ 23^{h}\ 33^{m}$. The 8th. year of Darius, according to the Aramaic reckoning, will be 416–5; and if we accept Professor Schürer's reading, the 9th. according to Egyptian reckoning will also be 416–5.

K. 23 (24) Shebat; 13 Darius = 8 (9) Athyr, 13 (14) Darius. The date here is clearly 10 Feb. 410, five years later than J., whether we begin the year in Nisan, in Tishri, or in Thoth. This makes it clear that 8, not 7, was the correct figure for the Aramaic year in J., so that the higher figures bracketed by Mr. Cowley are to be preferred to the lower figures in his text. The Aramaic 13 Darius and the Egyptian 14 Darius must both be 411–0, the former apparently beginning in Nisan,* the latter in Thoth. If 24 Shebat = 10 Feb., 1 Shebat will be 18 Jan., the month beginning at the sunset after the new moon of Jan. $17^{d}\ 3^{h}\ 9^{m}$.

This finishes the dates on the papyri edited by Professor Sayce and Mr. Cowley, but the data thus obtained enable us to date the remaining papyri more closely than would be otherwise possible. Tammuz in the 14th year of Darius, mentioned in the papyrus

* From the comment on B above, it appeared that the Aramaic years began somewhere before Elul.

edited by Professor Euting, and in the first and third of those edited by Professor Sachau, clearly belongs to 410 B.C., while 20 Marheshvan in the 17th year of Darius, on which the second and third of Professor Sachau's papyri are dated, just as clearly belongs to 407 B.C.

It may be well to arrange in parallel columns the dates proposed by Mr. Cowley, and those supported in the present article, to show how far the astronomical investigation affects the dates of the papyri.

Papyrus.	Mr. Cowley's Date.	Date now proposed.
A	471	471 Sept. 12
B	465	464 Jan. 2
C	459	460 Nov. 11 (?)
D	459	460 ,, 11
E	446	446 ,, 17 (19 ?)
F	440	440 Aug. 26
G	440	446 Oct. 14
H	421	420 Sept.
J	417	416 Dec. 16
K	411	410 Feb. 10

The next problem is to determine how far the dates obtained in the foregoing inquiry enable us to infer a theory of intercalation. Professor Schürer has calculated the date of 14 Nisan from each of the dates above. In order to compare better with Mr. Knobel's table, I have preferred to compute the new moon of Tishri, assuming in each case that Tishri is not preceded by an intercalary month, as it sometimes is in the Babylonian calendar. The dates given below are those of the astronomical new moon, as given in Professor Ginzel's tables.

Year B.C.	New Moon of Tishri.
471	23 Sept.
465	16 Oct.
460	23 Aug.
446	17 Sept.
440	10 Oct.
420	29 Sept.
416	14 Oct.
411	20 Sept.

All these dates except 23 Aug. 460 are consistent with a systematic intercalation. But even if we could abandon the August date, it would not follow that the intercalations were actually governed by rule, and not by the discretion of an authority possessing some astronomical knowledge. But the August date suggests that the intercalations were not regular. Professor Schürer thinks that they were determined on principles similar to

those which guided the sanhedrim at a later date when the weather and the state of the crops were considered as well as the course of the sun. For my own part, I cannot but think of the irregular intercalations of the Babylonian calendar, as proved by the contract tablets used by the late M. Oppert.* M. Oppert believed that the regular intercalations of the 19 years cycle were disturbed from time to time by the natural desire to prevent important astrological phenomena from falling on unlucky dates. Whatever the cause, the fact appears to be certain; and I should have inferred that the dates in these papyri were Babylonian but for a difficulty that will be mentioned later.

It will have been observed that, with two doubtful exceptions (E and F), all the lunar months in these papyri begin with the sunset following the new moon. The exception in E appears, as has been seen, to be due to an error in the papyrus, and when corrected, confirms the rule. If we substitute the mean new moon for the true new moon, we get rid of the exception in F. There we have 1 Ab = 13 August 440, with August $12^d\ 19^h\ 28^m$ as the date of new moon. Dr. Guinness † gives for the mean new moon August $12^d\ 14^h\ 7^m$, Jerusalem mean time reckoned from midnight, so that if mean new moons were the basis of this calendar and not true new moons, the exception would disappear. On the other hand, the mean new moon in K might possibly be a little too early. Dr. Guinness gives for this 410 January $16^d\ 17^h\ 23^m$, whereas 1 Shebat is 18 January. The date given by Dr. Guinness falls just after sunset at Jerusalem, but before sunset at Assuan; it must, however, be remembered that the modern Jewish calendar is calculated on the basis of a mean sunset at 6 p.m., and a calendar based on a mean new moon would probably also be based on a mean sunset.

Most of the modern values for lunar and solar constants would give a slightly earlier date. Reckoning by means of Oppolzer's tables with Hansen's constants, I get $4^h\ 25^m$ p.m. Assuan mean time; with Professor Ginzel's constants I get $4^h\ 48^m$, with Oppolzer's $5^h\ 8^m$, with Professor Newcomb's $5^h\ 10^m$, and with Mr. Cowell's $5^h\ 24^m$. The last of these would give $5^h\ 33^m$ p.m. for Jerusalem.

It is far from certain, however, how the compilers of an ancient calendar would reckon the mean new moon. The modern Jewish calendar would give $17^h\ 14^m$ (Jerusalem time) as the date of mean new moon on 16 January 410 B.C., but it is not likely that the mean new moons of the modern calendar are older than the great calendar reform of the fourth century A.D., though it is surprising that the date should be so accurate at such a distance of time.

* See his article, "La fixation exacte de la chronologie des derniers rois de Babylone," *Zeitschrift für Assyriologie*, 1893, pp. 56–74. Professor Ginzel gives a list of all known intercalary years in the Babylonian calendar, *Handbuch der Chronologie*, 1 pp. 133, 134. They clearly do not conform to a nineteen years' cycle.

† *Creation centred in Christ*, Astronomical Appendix (1896).

The question then arises whether it is possible to fix the mean new moon later than 6 p.m. on 16 January 410, without moving any of the other mean new moons from one day to the next, and, if so, what value the authors of this calendar used for the mean lunation. Of all the other mean new moons in the series earlier than 6 p.m., the one that comes nearest to that hour is the mean new moon of 21 October 460, which Dr. Guinness gives as $14^h\ 14^m$, and which the modern Jewish calendar dates $14^h\ 16^m$. This mean new moon need only be transferred to $15^h\ 2^m$, still well before sunset, if the mean new moon of 16 January 410 is to be transferred to 6 p.m. There is, therefore, no difficulty in supposing that the Aramaic months began at the sunset following mean new moon; and it is, of course, easier to suppose that those who had control of the calendar calculated the mean new moon than the true new moon. I have made a further investigation to see what duration of the synodical month is involved in these dates, on the supposition that no month begins before the mean new moon, and none more than twenty-four hours after the mean new moon. I find that these dates can only be reconciled with such a principle on the supposition that the synodical month was reckoned at not less than $29^d\ 12^h\ 43^m\ 53^s{\cdot}50$, and not more than $29^d\ 12^h\ 44^m\ 51^s{\cdot}15$. This calendar implies, therefore, a more exact value for the lunation than that adopted by the Greek astronomer Meton in 432 B.C. No such exact calculation seems to have been propounded in Greece before the time of Callippus, whose first cycle began in 330 B.C. But a value for the synodical month falling within the limits mentioned could be inferred at once from the eighteen years cycle of eclipses, and must have been known wherever that cycle was used. The knowledge necessary for the prediction of eclipses was possessed by Thales in 585 B.C., and must have existed at Babylon at an earlier date.

But this calendar is not Babylonian. All our evidence seems to show that the Babylonian months began with the first appearance of the crescent, though whether at the calculated or at the empirical date of the appearance is not so certain. In the Babylonian tables of appearances of the moon published by Epping,* the interval between new moon and the first appearance of the crescent varies from 18·8 hours to 52·2 hours, and only on two occasions out of thirty-three does the moon appear at the first sunset after the new moon. It follows that the months on these papyri generally began one day earlier than the Babylonian months. But if the calendar was not Babylonian, neither was it the same as that used by the Jews in the age preceding the Mishna. The Jews of that period found the beginning of the month by simple observation, and therefore this theory, though maintained by Professor Schürer, is open to the same objection as that which would regard the dates as Babylonian. The calendar rules suggested by Mr. Knobel will not hold, because in only two instances do they give exactly the same dates as those of the papyri. I have tested the modern

* *Astronomisches aus Babylon* (1889), pp. 18–24.

Jewish rules, by which each day of the year can only fall on one or other of certain days of the week, and by which the different months, with the exception of Marheshvan and Chisleu have fixed durations, Tishri alone being computed directly from the mean new moon; and I find that neither of these rules will apply to the calendar dates before us. It remains that these dates belong to a hitherto unknown calendar, where intercalation appears to be more or less arbitrary, but where the length of each month is rigidly fixed by the rule that each begins at the sunset after the mean new moon. The mean new moon may have been simply calculated from an astronomical value, or a cycle may have been framed which would give effect to the rule. The shortest such cycle, consistent with the length found above for the synodical month, would be one of 49 months, based on a value (as it happens, a very accurate value) of $29\frac{26}{49}$ days for the lunation. Such a cycle would be composed of two periods of 17 months and one of 15. If we arrange each of these periods with months of 29 and 30 days alternately, beginning each period with a 29-day month, and giving the last month of each period 30 days instead of 29, and if we place the 15-month period last of the three, the calendar dates of these papyri will be found to accord with such a cycle, on the assumption that the first month mentioned on our papyri, Elul 471 B.C., is the 6th, 8th, 23rd, 25th, 38th, 40th, or 42nd of the cycle. Working with such a cycle, and assuming that the new moon of Tishri 407 B.C., like all but one of the Aramaic Tishris that we have been able to date, falls not earlier than 17 September, nor later than 16 October, we find that 20 Marheshvan 407 in the second and third of Professor Sachau's papyri, the one exact Aramaic date which is given without a corresponding Egyptian date, will be either 24 November or 25 November 407 B.C.

This calendar, whether its dates were computed by direct astronomical calculation or by a lunar cycle, is clearly much more scientific than the merely empirical rules used by the Jews of the first and second centuries of our era. If this was the calendar of the Jews of Palestine, their calendar must have afterwards developed in a retrograde direction. It seems easier to suppose that as the Jews of Elephantine had a temple of their own, they had their own council of priests or elders who regulated the beginning of the month by strict rules and the beginning of the year according to their own discretion. Whether the astronomical knowledge involved was acquired from Egypt or from Babylon, I cannot say. We have not, so far as I know, any evidence as to the Egyptian value for the synodical month at the date to which these papyri belong.

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Oppolzer's and Ginzel's Corrections to Hansen.

By J. K. Fotheringham.

Oppolzer, in his *Syzygientafeln* (1881), p. 15, proposes certain corrections to Hansen's values for the mean motions and accelerations of the Moon. As these corrections have been applied in the calculation of Oppolzer's *Canon der Finsternisse* (1887), it is important that they should be correctly interpreted by those who have occasion to use the *Canon der Finsternisse*.

Oppolzer gives his corrections in the form—

$$\begin{array}{rlll} \Delta T = & +0^{\circ}0006\, s & +0^{\circ}00009\, s^2 & +0^{\circ}000\ 00009\, s^3 \\ \Delta(g+\omega) = & -0^{\circ}019\, s & -0^{\circ}0004\, s^2 & -0^{\circ}000\ 0004\, s^3 \\ \Delta g = & & +0^{\circ}003\, s^2 & +0^{\circ}000\ 003\, s^3 \end{array}$$

where s is the interval in centuries since 1800·0,
T is the time of mean syzygy expressed in decimals of a day,
g is the mean anomaly of the Moon,
ω is the longitude of lunar perigee measured from the ascending node.

As Oppolzer's tables exist for the purpose of computing the elements of a syzygy, not of constructing an ephemeris, his corrections naturally apply to the moment of mean syzygy, not to a fixed moment of time; further, since his tables express g in centesimal degrees, and $g+\omega$ in sexagesimal degrees, it seems reasonable to suppose that the corrections are expressed in the same form. In order to make sure that these principles of interpretation are correct, I have computed the corrections for −101 and −462, and find that they only agree with the corrections actually applied if interpreted as described above. Unfortunately, Oppolzer gives no warning as to the interpretation of his corrections, and they have in consequence been frequently misunderstood. The misunderstanding is rendered the easier by the use of the symbol ° for centesimal degrees. I have not found any other passage in Oppolzer where that symbol is used for any but sexagesimal degrees.

Professor Ginzel, in his *Astronomische Untersuchungen über Finsternisse* in *Sitzungsberichte der kaiserlichen Akademie der Wissenschaften math. naturw. Classe*, lxxxix. (2), (1884), uses Oppolzer's *Syzygientafeln* as the basis of his corrections, and, while realising that the corrections are to be applied to the moment of mean syzygy, interprets Δg as if it were expressed in sexagesimal degrees. The corrections thus obtained are, however, tested by the eclipses used and made the basis of further corrections, which are not affected by the misunderstanding of Oppolzer's Δg. Dr. Schram, in his *Reductionstafeln* (*Denkschriften der k. Akademie der W. math. naturw. cl.*, lvi.) (1889), in reducing Professor Ginzel's

corrections so as to be more directly applicable to Oppolzer's *Canon*, interprets Oppolzer's corrections rightly; and, as Professor Ginzel's *Spezieller Kanon* (1899) is based on Dr. Schram's *Reductionstafeln*, it is not affected by Professor Ginzel's error in the interpretation of Oppolzer.

When Oppolzer's own collaborator, Professor Ginzel, misunderstood him, it is not unnatural that he should have been misunderstood to an even greater extent by others. Professor Newcomb, in *Astronomische Nachrichten*, 154 (1901), pp. 197–202, discusses Oppolzer's and Ginzel's corrections and appears to misunderstand both. Professor Ginzel expresses his corrections in a form similar to Oppolzer's. Those corresponding to Oppolzer's corrections are expressed as follows:—

$$\Delta T = -0^{\circ}000192\, s + 0^{\circ}0000247\left(1 + \frac{s}{1000}\right)s^2$$

$$\Delta P = \Delta Q = -0^{\circ}00769\, s + 0^{\circ}000049\left(1 + \frac{s}{1000}\right)s^2$$

$$\Delta I = +0^{\circ}0240\, s + 0^{\circ}00052\left(1 + \frac{s}{1000}\right)s^2$$

T and s are clearly intended to have the same meaning here as in Oppolzer, though Professor Ginzel (*Spezieller Kanon*, p. 5) defines s as the number of years since 1800·0; ΔP is the same as Oppolzer's $\Delta(g+\omega)$ and ΔI is the same as Oppolzer's Δg and is expressed in centesimal degrees. These corrections were intended by Professor Ginzel to be substituted for Oppolzer's. Professor Newcomb, in the article to which I have referred, reduces both Oppolzer's and Ginzel's corrections to the form of corrections of the longitude of the Moon and of its ascending node and perigee. He reduces ΔT correctly into a correction of the moon's longitude (ΔL) for a given time by multiplying it by the factor $-43900''$. He then deducts from this correction Oppolzer's $\Delta(g+\omega)$ and Professor Ginzel's ΔP respectively and calls the remainder $\Delta\Omega$. This would be legitimate if $\Delta(g+\omega)$ and ΔP referred to a fixed moment of time; but since they refer to the moment of mean syzygy, it is necessary to deduct from $\Delta(g+\omega)$ and ΔP respectively the motion of $(g+\omega)$ between Hansen's and the corrected moment of mean syzygy before comparing $\Delta(g+\omega)$ with ΔL. Professor Newcomb next interprets Oppolzer's Δg and Professor Ginzel's ΔI as referring to sexagesimal degrees, deducts each from ΔL, and calls the remainders $\Delta\pi$. Then he attacks the corrections of perigee and node thus computed, asserting that the corrections of the centennial motions are incompatible with the observations of the last 200 years, and that the corrections of the accelerations are incompatible with gravitational theory. This result is a little surprising in the case of Professor Ginzel's secular accelerations of the node and perigee, which profess to be taken direct from Delaunay.

Professor's Newcomb's reduction of Oppolzer's and Ginzel's corrections is as follows:—

	Oppolzer.		Ginzel.	
$\Delta L =$	$-26''.3\,s$	$-3''.95\,s^2$	$+8''.4\,s$	$-1''.08\,s^2$
$\Delta \Omega =$	$+42\,\ \ s$	$-2''.5\,\ s^2$	$+36''.1\,s$	$-1''.26\,s^2$
$\Delta \pi =$	$-26\,\ \ s$	$-14''.7\,\ s^2$	$-78''.0\,s$	$-2''.95\,s^2$

Allowing for the motion of the Moon and its perigee and node between Hansen's and the corrected moment of mean syzygy, and converting Δg and ΔI from the centesimal to the sexagesimal system, I reduce as follows:—

	Oppolzer.		Ginzel.	
$\Delta L =$	$-26''.3\,s$	$-3''.95\,s^2$	$+8''.4\,s$	$-1''.08\,s^2$
$\Delta \Omega =$	$+71\,\ \ s$	$+1''.8\,\ s^2$	$+27''.0\,s$	$-0''.08\,s^2$
$\Delta \pi =$	$+\ 2\,\ \ s$	$-9''.4\,\ s^2$	$-78''.4\,s$	$-1''.61\,s^2$

From this it will appear that Oppolzer's correction to the centennial motion of perigee falls within the limit (10″ or a little more) of error which Professor Newcomb allows, though his other corrections are still open to objection. Professor Ginzel's corrections of the centennial motions of node and perigee are, as Professor Newcomb says, incompatible with modern observations, but it will be observed that his corrections of the accelerations of node and perigee are in exact agreement with gravitational theory, while his correction of the centennial motion of the argument of latitude $(L - \Omega)$ is at least superior to the correction adopted in the *Nautical Almanac*. It would appear, then, that Professor Newcomb has overstated the case against Oppolzer and Ginzel, but that he is right in regarding some of their corrections as impossible.

In order to render the effect of these corrections the clearer, I append the lunar and solar formulæ which result from them.

Oppolzer.

$g =$	110°	19′	33″.64 +	171791	$5808''.93\,T + 56''.623\,T^2 + 0''.056639\,T^3$
$\omega =$	192	7	27′.41 +	2161	$1442'.28\,T - 56'.417\,T^2 - 0'.056432\,T^3$
$\Omega =$	33	16	25′.65 −	696	$2857'.94\,T + 9'.522\,T^2 + 0'.008403\,T^3$
$L' =$	279	54	49′.21 +	12960	$2766'.73\,T + 1'.1073\,T^2$
$\pi' =$	279	30	17′.35 +		$6168'.13\,T + 1'.823\,T^2$

Ginzel.

$g =$	110°	19′	33″.64 +	171791	$5923''.93\,T + 51''.657\,T^2 + 0''.051673\,T^3$
$\omega =$	192	7	27′.41 +	2161	$1405'.75\,T - 46'.723\,T^2 - 0'.046738\,T^3$
$\Omega =$	33	16	25′.65 −	696	$2901'.65\,T + 7'.660\,T^2 + 0'.006541\,T^3$
$L' =$	279	54	49′.21 +	12960	$2766'.73\,T + 1'.1073\,T^2$
$\pi' =$	279	30	17′.35 +		$6168'.13\,T + 1'.823\,T^2$

As Oppolzer treats as negligible the change in the mean anomaly of the Sun between Hansen's mean syzygy and his own, the values actually represented by his tables imply a corresponding correction to the solar perigee. This would make

$$\pi' = 279^\circ\ 30'\ 17''{\cdot}35 + 6170''{\cdot}26\,T + 2''{\cdot}142\,T^2 + 0''{\cdot}000319\,T^3$$

No such correction is necessary in the case of Professor Ginzel's values, since he has introduced corrections which make the neccssary allowance for the change in the Sun's mean anomaly.

Mr. Nevill, in *Monthly Notices*, lxvii. p. 14, explains that in his recent discussion of ancient eclipses he worked with Oppolzer's tables, applying certain corrections, and he proceeds to give corrections to Hansen's tables, to which the elements employed by Oppolzer are stated to correspond. His reductions are obtained in the same way as those given by Professor Newcomb, but they are expressed as corrections of the elements in Hansen's tables, not of the corrected elements, mainly from the *Darlegung*, used by Oppolzer. To these Mr. Nevill applies further corrections in order to bring the centennial motions into accord with modern observations, and the secular accelerations into accord with theory.

He thus obtains the following corrections to be applied to Oppolzer :—

$$\text{(Mean longitude)}\ \Delta\zeta = +0''{\cdot}00 - 3''{\cdot}66\,T - 2''{\cdot}496\,T^2$$
$$\text{(Longitude of perigee)}\ \Delta A = +0''{\cdot}00 + 37''{\cdot}14\,T + 13''{\cdot}128\,T^2$$
$$\text{(Longitude of node)}\ \Delta B = +0''{\cdot}00 - 48''{\cdot}06\,T + 2''{\cdot}672\,T^2$$

Accepting Mr. Nevill's values for the results of modern observations and theory, I find that these corrections should have been —

$$\Delta\zeta = +0''{\cdot}00 - 3''{\cdot}66\,T - 2''{\cdot}496\,T^2$$
$$\Delta A = +0''{\cdot}00 + 8''{\cdot}92\,T + 7''{\cdot}817\,T^2$$
$$\Delta B = +0''{\cdot}00 - 76''{\cdot}67\,T - 1''{\cdot}617\,T^2$$

The effect of Mr. Nevill's misunderstanding of Oppolzer is that he has unconsciously applied a further correction :—

$$\Delta A = +28''{\cdot}22\,T + 5''{\cdot}311\,T^2$$
$$\Delta B = +28''{\cdot}61\,T + 4''{\cdot}289\,T^2$$

With the corrections obtained in this manner Mr. Nevill calculates, with the aid of Oppolzer's *Canon*, the elements of a number of eclipses, and gives the result as that which accords with modern theory. He also reduces three other sets of lunar and solar values, including Mr. Cowell's, to the form of corrections to be applied to Oppolzer, interpreting Oppolzer as before, and therefore making the same error in the reduction. It is no wonder that Mr. Cowell

should complain that the results which Mr. Nevill deduces from his values differ widely from those which he deduces himself. As Mr. Nevill's error is a large one, it seems to follow that his calculations must be set aside, and if different theories of lunar motion are to be tested by ancient eclipses, the work will have to be done afresh.

It does not appear to me that the errors in Professor Ginzel's centennial motions are a sufficient reason for setting aside his elaborate computations of ancient eclipses. If a set of values, consistent with modern observation and with theory, would give the same results for ancient times, Professor Ginzel's computations would be evidence in favour of such a set of values. The most important elements in eclipse calculations are elongation (D) and the argument of latitude (F). Professor Ginzel's mean motion of D exceeds Mr. Cowell's (based on observation) by 38″, and his mean motion of F exceeds Mr. Cowell's by 20″. If we substitute Mr. Cowell's mean motions for Professor Ginzel's, we may, by making corresponding reductions in Professor Ginzel's secular terms, still obtain approximately the same results for ancient eclipses. Taking −300 as a mean date for the ancient eclipses falling under discussion, we must reduce Professor Ginzel's coefficients of the secular terms of D and F by $\frac{38''}{21} = 1''{\cdot}8$ and $\frac{20''}{21} = 0''{\cdot}9$ respectively. This gives $+9''{\cdot}7$ for the secular coefficient of D and $+4''{\cdot}0$ for the secular coefficient of F. The resultant secular coefficient of D − F ($5''{\cdot}7$) does not differ greatly from the theoretical coefficient ($6''{\cdot}55$). It is not likely that any one would propose a secular acceleration of the Sun to account for so small a difference, for it ought not to be difficult to make small re-arrangements of the different values so as to bring this coefficient within the range permitted by modern theory without seriously affecting the result in ancient times. It may therefore be said that Professor Ginzel's elements of ancient eclipses roughly represent modern observation and theory for the motion of the node and mean motion of the Moon, with a value of $9''{\cdot}7$ for secular acceleration; and if his tables should be found to do justice to the historical data, there would be no case for a secular acceleration of the Sun. Mr. Cowell's values have, so far as they have been tried, yielded better results, but it would be interesting to have the two sets of values compared more thoroughly than has yet been done.

1908 *November* 7.

Historical Data for the Secular Acceleration of the Moon.

By J. K. Fotheringham.

Father Kugler, in his recent volume on Babylonian Astronomy,* compares the new moon longitudes given in the lunar tables of Bania and Marduk-tabik-ziru constructed in −102 with the longitudes computed by means of Oppolzer's tables, and concludes that Oppolzer's new moon longitudes are on an average 1° 2′ too high. The accuracy of the constants employed by Bania and Marduk-tabik-ziru is so great, and the observations on which these tables were based were presumably so nearly contemporary, that Father Kugler infers that at least the greater part of the error must lie in the modern, and not in the Babylonian tables. Now, it is incredible that any error approaching this can exist in our modern tables, whose substantial accuracy is confirmed by numerous ancient eclipses, and I have therefore thought it worth while to make a fuller analysis of the tables of Bania and Marduk-tabik-ziru, especially as these, if accurate, ought to furnish us with corrections not only to the Moon's longitude, but to her perigee and node, as also to the longitude and perigee of the Sun.

The most important part of these tables will be found in Father Kugler's *Babylonische Mondrechnung* (1900), pp. 12, 13, where they are accompanied by a very full discussion. The tables are dated at Sippara, −102 December 22, and extend from −103 March 22 to −100 April 17. The lunar constants are, as Father Kugler has shown, identical with those used by Hipparchus, but the Babylonian solar constants appear to be less accurate than those of the Greek astronomer. Father Kugler has computed the sidereal, synodical, anomalistic, and nodical months, and also the sidereal and anomalistic years, by means of these tables, and compares the result with their values in 1800·0, but he has not computed any places except the longitudes of new moon. I have preferred to deduce from these tables the centennial motions and mean places at the epoch −101 December 20·0, G.M.T., exactly 1900 years before the epoch of Hansen's tables, and to compute the secular accelerations which result from the comparison of these with the centennial motions and mean places deduced by Mr. Cowell from modern observations, and published in *Monthly Notices*, lxv. p. 863. As the Babylonian mean motions are sidereal, I have subjected Mr. Cowell's centennial motions to a correction for precession, and in computing the Babylonian places I have provisionally accepted Father Kugler's value for the longitude of the Babylonian first point of Aries. This, reduced to the epoch

* *Sternkunde und Sterndienst in Babel*, i. Buch, *Babylonische Planetenkunde* (Münster, 1907), pp. 172, 173.

that I have used, works out at 355° 40′·5. I thus obtain the following results :—

	Bania and Marduk-tabik-ziru. Epoch − 101 December 20·0.					Cowell. Epoch 1800·0.				
g	297°	47′	36″ +	171791	4714″·72 T	110°	19′	38″ +	171791	5794″ T
ω	248	38	43 +	2161	2559·25 T	192	7	25 +	2161	1516 T
☊	57	2	42 −	696	8034·05 T	33	16	21 −	696	7944 T
L′	264	10	43 +	12959	6618·76 T	279	54	29 +	12959	7743 T
π'	255	30	41 +		2980·18 T	279	29	47 +		1163 T

By comparing these two sets of values, I obtain—

	Secular Acceleration resulting from B. and M.'s Places.	Secular Acceleration resulting from B. and M.'s Mean Motions.
g	= + 52″·5 T²	+ 28″·4 T²
ω	= − 26·9 T²	− 27·5 T²
☊ (sidereal)	= − 50·7 T²	+ 2·4 T²
L′ (sidereal)	= − 12·4 T²	+ 29·6 T²
π' (sidereal)	= + 87·5 T²	− 47·8 T²

It will be observed that, of the places, all except g yield absolutely impossible secular accelerations, while of the mean motions $g + \omega$ alone yields a possible acceleration. It is highly improbable that the Babylonian astronomers could accurately determine the mean anomaly of the Moon, and not the other elements in its position; and the accuracy of the mean anomaly must therefore be regarded as accidental. The tables are surprisingly accurate, considering the conditions under which ancient observations were made and the difficulties attending lunar theories; but they are useless for the purpose of correcting modern values.

This is the only attempt that Father Kugler makes to deal with secular acceleration. There is, however, a certain amount of material in his recent volume which might conceivably be used for the purpose. Thus on p. 24 he quotes a Babylonian observation of a lunar eclipse, followed at an interval of 14 days by a solar eclipse. The record runs as follows: "In the year 175, Adar II., on the night of the 15th (− 135 March 31) an eclipse of the moon on the south-east side of seven digits magnitude took place . . . . on the 29th (− 135 April 14) an eclipse of the sun on the south-west side." I have not computed the lunar eclipse, to which Professor Ginzel's tables assign a magnitude of 8·8. It adds one to the list of ancient eclipses with definitely recorded magnitudes. The solar eclipse was total at Babylon, according to Professor Ginzel's tables. I have computed it with Mr. Cowell's values, and find an eclipse of the north limb with a magnitude of 10·61. If the description implied that the eclipse was on the south limb it would be fatal to Mr. Cowell's values, but, as the eclipse is at the ascending node, the uneclipsed portion of the Sun

must have been either on the north-west or on the south-east, and the statement that the eclipse was on the south-west side must, apparently, refer to the beginning, and not to the greatest phase of the eclipse. If so, it would be satisfied by any possible combination of lunar and solar values, and is useless for the correction of secular acceleration.

Father Kugler also publishes several tablets giving observed places of the Moon and planets. I have not tested these, and am not prepared to say whether they are sufficiently accurate to be of use for the correction of modern values.

Passing from Babylonian to Greek and Roman data, I have made a statistical account of recorded solar eclipses, classifying them according as they are total, annular, or annular-total on the Earth generally, and according as they are or are not clearly stated or implied to be partial in the records. I only include those eclipses which have been identified with a high degree of probability, and I take them entirely from Professor Ginzel's *Spezieller Kanon*, with the exception of the eclipse of −462, which I have discussed in *The Observatory* (November 1908, pp. 399–402). They thus represent the eclipses recorded by Greek and Latin writers before 600 A.D. I find that of the recorded eclipses, sixteen (−430, −423, −403, −393, −93, −30, +186, +218, +316, +324 or 326, +334, +360, +464, +497, +563, +590) were annular; thirty-one (−647, −584, −462, −399, −363, −309, −216, −189, −187, −35, +29, +45, +59, +240, +292, +319, +346, +348, +364, +393, +402, +418, +447, +458, +484, +495, +512, +538, +540, +547, +592) were total; and four (−202, −103, +5, +71) were annular-total. Now there should, on an average, be 13 total to every 16 annular eclipses. We have therefore an excess of 18 total eclipses, which is most easily explained by the supposition that the majority of the total eclipses locally attained a magnitude greater than that of an annular eclipse. This presumption is rendered the stronger when we observe that of the 16 annular eclipses six (−430, −423, −393, +464, +563, +590) are described in the records as partial. The same applies to two (−202, +5) of the four annular-total eclipses, but to not more than three (−216, +458, +495) of the 31 total eclipses. Now there is no reason why, among eclipses locally partial, a much larger proportion of those annular on the Earth generally than of those total on the Earth generally should be recorded as partial. The only reason that I can see why the partial character of the annular and annular-total eclipses should have been more frequently mentioned than that of the total eclipses is, that a very large proportion of the latter were locally as well as generally total. It may therefore be inferred that a set of lunar and solar values must not only satisfy the eclipses recorded as total at particular places, but a large proportion of other eclipses. Professor Ginzel's values, which are not based on any eclipses in this list except those of −584, −309, +29, +71, render the following additional eclipses total at the supposed

places of observation: −647, −584, −399 (after sunset), +393, +447, +484; they also assign to the following total eclipses a local magnitude of 11 digits or more: −462, −187, +292, +418, +512, +540. Among the annular eclipses +590 attains the same magnitude. We thus have 15 or 16 total eclipses, 1 annular-total, and 1 annular eclipse exceeding 11 digits magnitude at the place of observation. This is not far short of the proportion that might be expected. It would be interesting to see whether Mr. Cowell's values, which have yielded more satisfactory results in the cases to which they have been applied, would give as large a proportion of total or nearly total eclipses. The eclipses which cannot be localised and those to which Professor Ginzel assigns a magnitude of less than 10 digits might be omitted. This leaves the following eclipses, to which Mr. Cowell's values have not yet been applied:—

Date.	Probable Place of Observation.
−393	Chæronea.
−189	Rome.
−187	Rome.
−30	Rome.
+29	Nicæa.
+59	Rome and Baiæ (?).
+71 (immediately after noon)	Chæronea.
+186	Rome.
+218	Rome.
+292	Rome.
+319	Rome.
+334	Sicily.
+346 (stars seen)	Constantinople.
+393	Constantinople. Ravenna (?).
+402	Ravenna.
+418 (stars seen and perhaps corona)	Constantinople.
+447	Chiaves.
+484 (total)	Athens.
+512	Constantinople.
+540	Constantinople (?).
+590	Constantinople.

In the case of the eclipse of +364, otherwise unimportant, the exact time of the beginning, middle, and end of the eclipse is given us.

I append a computation which I have made of the eclipse of −462 with Mr. Cowell's values and formulæ. Mr. Cowell has kindly checked my computation.

T.	T^2.	T^3.	g.	ω.	$-\Omega$.	L′.	π'.	
−22·616398	511·49	−11568	38° 32′ 44″	135° 4′ 45″	142° 17′ 0″	32° 56′ 31″	240° 49′ 21″·5	31°

V−L.	V′−L′.	V−V′.	U.	$\frac{d}{dt}(V-V')$.	$\frac{dU}{dt}$.	$p-p'$.
+11171″	+3341″	+2048″	+952″	1816″·5	−179″	+361

Place.	Lat. N.	Long. E.	Date.	Local Mean Time in degrees.	h = Local Sidereal Time in degrees.	v'.
Athens.	37° 56′	23° 38′	−462 April 30	46° 331	79° 2725	+743·5+1945−111=+2

u'	$\frac{dv'}{dt}$.	$\frac{du'}{dt}$.	$\frac{d}{dt}(V-V'-v')$ denom. of k.	$\frac{d}{dt}(U-u')$ num. of k.	k.	Resi $k(V-(U$
+2033−1129=+904″	+440+11=+451″	−49″	+1365″·5	−130″	−0·095	

The eclipse is therefore total at Athens.

1908 *November* 9.

The Total Solar Eclipse of 1911 *April* 28.

By A. M. W. Downing, D.Sc., F.R.S.

As inquiries have already been addressed to me with regard to this eclipse, it may perhaps be convenient to publish, thus early, some details respecting it which I have collected.

The most suitable place for observation appears to be Vavau, one of the Tonga Islands. This island is nearly on the central line of the shadow, and is otherwise a most convenient observing station. The following particulars with regard to it are extracted from the *Admiralty Sailing Directions*, *Pacific Islands*, vol. ii., 1908.

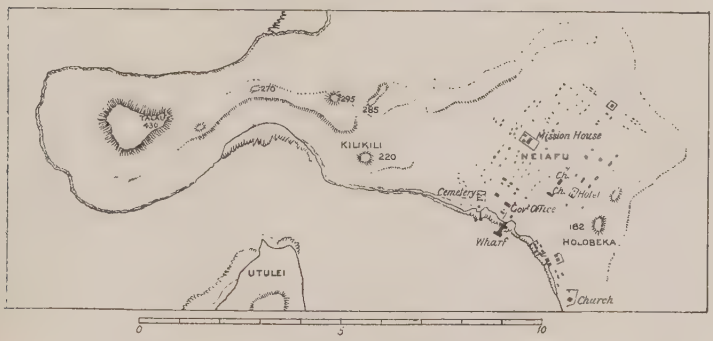
"Vavau Island is nearly $9\frac{1}{2}$ miles in length east and west, with an extreme breadth from north to south of $6\frac{3}{4}$ miles. The island is covered with thick bush and cocoanut trees, with numerous patches of land under cultivation."

"Neiafu, situated on the south-west coast of Vavau, one mile eastward of Talau Hill (see plan), is the port of call for the mail steamers, which call here from Sydney every four weeks. The Governor resides here, and it is the headquarters of some English and German trading firms. There is a good wharf, which vessels of 18 feet draught can go alongside."

"The country affords plenty of vegetables and fruit. Water

in wells is scarce and indifferent; rainwater has to be collected and stored in tanks. Labour is difficult to obtain; the natives will not work under a dollar a day. The traders import labour from other Pacific islands."

The plan of Neiafu and its environs, here reproduced, is taken from the official chart published by the Hydrographic Office.



10 cables = 1 sea mile.

Neiafu, Vavau Island.

The following astronomical particulars of the eclipse, as seen from Neiafu, are computed from the data of the *Nautical Almanac*. For the convenience of observers, I have added the intervals in time from the instant when the cusps subtend an angle of specified value at the Sun's centre to the commencement of totality. It is to be noted that, with the existing errors of the lunar tables, the times of the contacts given below are, in each case, several seconds too late.

NEIAFU.—*Long.* 173° 59′ W. *Lat.* 18° 39′ S.

Local Mean Time.

	d	h	m	s	Angle from North Point.	Angle from Vertex.
First contact . .	April 27	20	26	47	230°	350°
Total eclipse . .	27	21	37	1·7	36	169
	27	21	40	38·5	242	15
Last contact . .	27	22	58	48	49	203

	m	s
Duration of totality . . .	3	36·8
Sun's altitude at totality . . .		43°
Sun's azimuth (from N.) at totality .		49

Angle of Cusps.	Time before Commencement of Totality.
°	s
90	42
60	15
45	9
30	4
15	1

I append some details of a meteorological character, extracted from the volume of *Sailing Directions* referred to above, which may be useful to observers as affording an indication of the probable weather conditions at the time of the eclipse. They are deduced from observations taken at Neiafu during the years 1890–91. The "amount of cloud" given below is not published in the *Sailing Directions*, but has been kindly communicated to me by Dr. W. N. Shaw, F.R.S., having been inferred from the weather records obtained at Neiafu during the above-mentioned years, which are preserved in the Meteorological Office.

Month.	Mean Temperature.		Rain.		Amount of Cloud at 9 A.M. (Scale 0–10).
	Max.	Min.	Total Fall.	No. of Days.	
	°	°	in.		
April . .	85	73	9·24	18	6·5
May . .	83	71	3·04	9	5·7

Prevailing direction of wind at 9 A.M., S.E.

Average force (Beaufort scale) ,, 4

H.M. Nautical Almanac Office:
1908 *November* 6.

Observations of Jupiter during the Apparition of 1907–8.

By the Rev. T. E. R. Phillips. (Plate 1.)

The planet Jupiter was in "opposition" on 1908 January 29, in R.A. $8^h\ 45^m\ 0^s$, Dec. 18° 52′ 48″ N., in the constellation Cancer. It was therefore very favourably placed for observers in the N. hemisphere, and between the latter part of September and the second week in June I was able to make observations on 126 occasions. These consisted of (1) notes and drawings of the forms and colours of the surface markings; (2) transit observations of spots in various latitudes across the central meridian of the illuminated disc; and (3) a few notes of special satellite phenomena.

All these observations were made with the 12¼-inch equatorial reflector that belonged to the late Dr. W. A. Kibbler, which instrument was substituted in 1907 September for the 9¼-inch reflector that had been in use for some years. The mirror of the Kibbler telescope—one of With's "reserves"—was re-figured a few years ago by Mr. Calver.

The following is a summary of the principal points of interest presented by the observations:—

(1) *Surface Markings.*

There were no very striking changes—at any rate at the beginning of the apparition—as compared with the planet's appearance in 1906–7, and the general aspect of the disc will be seen from the accompanying drawings.

The N. Equatorial Belt, which had broadened so remarkably the previous year, was at first still obviously triple, and for several months retained its distinctive colouring. The southern part of the belt was an intense brick-red, which merged gradually into orange and yellow in the central regions, and the narrow band which formed the north edge was blue. But during the latter part of the apparition the red and orange seemed to fade somewhat rapidly, and by the end of April the colour of the central portions of the belt had diminished to such an extent that the above-mentioned blue band seemed quite a separate object, and might well be taken for the N. Temperate Belt. It should be said that although there were still a number of dark streaks and white spots and gaps at the S. edge of the belt, they were generally far less conspicuous than in 1906–7. On the other hand, there were some interesting details in the middle portion, which (especially near the longitude of the Red Spot) exhibited rapid and somewhat puzzling changes.

The Equatorial Zone was again mottled with numerous light and dusky regions, and as usual was crossed by a series of delicate wisps.

The S. Equatorial Belt, as in 1906–7, was brownish on the whole. In some longitudes it appeared almost of a neutral tint, and there was still a purplish tinge at times over the portion of the belt in and near the f. "shoulder" of the Red Spot bay. As the apparition advanced, the white rift dividing the belt longitudinally became remarkably sinuous, and swollen into a series of rather large and very brilliant spots.

The Great Red Spot was at first by no means a really difficult object, and appeared as a dusky grey ellipse with rather pointed ends, and a somewhat lighter central region. But later, after the conjunction of the great S. tropical disturbance had begun, it seemed to lose its clearness of outline, and to become indistinct and misshapen, as if confused and distorted by overlying clouds. White spots were also seen at its *p.* and *f.* ends. A similar effect was observed at the previous conjunction of 1906, though the complete oval form of the hollow at such times is always conspicuously displayed. It is worth noting that the region of the S. Equatorial Belt near the *p.* "shoulder," which has for many years been marked by a widening of the longitudinal rift, and (particularly in 1906–7) by an extreme faintness of the S. component of the belt, during the past apparition became very dark and strong like the region about the *f.* "shoulder."

The Great S. Tropical Disturbance has lost none of its characteristic features, and continues to be one of the most interesting of the surface markings. Just at the beginning of the observations it seemed to have become fainter, and to be breaking up into dusky wisps stretching across the zone, but this was only a temporary appearance, and has been observed on sundry previous occasions. The *p.* end of the disturbance overtook the *f.* "shoulder" of the Red Spot hollow just before the middle of January 1908, and it is noteworthy that on this occasion the edge of the dark matter was observed for the first time to encroach on the *S.f.* margin of the Red Spot itself, and where it joined the S. Equatorial Belt, to extend distinctly some little distance beyond the great "shoulder," which was now and then clearly seen through it. In length, the dark region increased from a little less than 70° at the beginning of the apparition to 100° ± at the end of May, and thus exceeded all previous measurements.

The N.N. Temperate Belt and the *N. Polar Regions*, which had been bluish-purple in 1906–7, were again brown, the N.N. Temperate Belt even showing at times a decided tinge of red.

The S. Temperate Belt was bluish-grey, and the region from the S.S. Temperate Belt to the S. Pole grey.

(2) *Rotation Periods.*

As in previous years, a good deal of attention has been given to the drift of spots, and the determination of their rotation periods. About 1600 transit observations of various markings over the central meridian were secured during the past apparition, and I

have to acknowledge the assistance of Miss Church of Ashtead in the somewhat heavy work of their reduction. On plotting the observations it was found that just over 100 separate objects had been observed sufficiently well to give results that are reasonably reliable; and where gaps in the series or other difficulties have caused some uncertainty, the fact has been duly noted in the remarks which follow. The spots are obviously too numerous to give in detail, but the mean values of their periods will be found in the accompanying table.

Table of Rotation Periods.

	Objects observed.	No. of Spots.	Mean No. of Observations.	Mean No. of Rotations.	Rotation Period. h m s
	Spots in South Polar Regions . .	2	6	124	9 55 24·3
	South South Temperate spots . .	8	5·5	158	9 55 4·9
	Bright and dark spots on and at S. edge of South Temperate Belt . .	21	12	345	9 55 20·4
Great South Tropical Disturbance.	*Preceding* white spot . . .	1	13	632	9 55 16·3
	Preceding end of dark matter at S. Temp. Belt	1	25	632	9 55 16·3
	Preceding end of dark matter at S. Equatorial Belt . . .	1	26	632	9 55 16·3
	White spot in S. Tropical Disturbance	1	7	380	9 55 22·4
	Following end of dark matter at S. Temp. Belt	1	25	632	9 55 22·5
	Following end of dark matter at S. Equatorial Belt . . .	1	33	632	9 55 22·7
	Following white spot . . .	1	20	494	9 55 22·2
	Mean of *p.* and *f.* ends of the Disturbance	...	...	...	9 55 19·4
	Great Red Spot Hollow . . .	1	43	603	9 55 40·7
	White spots in middle of S. Equatorial Belt	3	6	254	9 55 33·7
	White and dark spots at and near N. edge of S. Equatorial Belt and on Equatorial Zone	42	15	391	9 50 26·7
	White and dark spots at S. edge of N. Equatorial Belt	7	6	245	9 50 33·0
	Dark spots in and at N. edge of N. Equatorial Belt	5	15	359	9 55 22·0
	Spots on N.N. Temperate Belt . .	5	7	255	9 55 37·4
	Spots in N. Polar shading . . ,	2	4	177	9 55 42·3

Remarks on the Rotation Periods.

Almost all the results given in the above table indicate a slight increase of velocity as compared with the corresponding values for last year, and in some instances the objects have been so well observed in both years as to leave no room for doubt as to the reality of the acceleration.

Spots in S. Polar Regions.—The observation of markings in these high latitudes is a matter of considerable difficulty, partly in consequence of foreshortening, and partly because of the slowness of their linear motion.

The transit times secured are therefore not quite so accordant as could be wished, and the rotation period given in the table cannot be regarded as more than approximate.

Spots on and at S. edge of S. Temperate Belt.—In 1906–7 the S. temperate current was drifting at a perceptibly slower rate than usual. In the last apparition a slight acceleration seems to have set in.

The Great South Tropical Disturbance.—As in former years, the periods of the *p.* and *f.* ends were not the same, but whereas in 1906–7 the motion of the *f.* end was the more rapid of the two, the reverse was the case in 1907–8, when the difference of rate amounted to rather over 6^s. The rate of the *p.* end, however, showed some striking variations. Thus from September 23 to January 20 the period was $9^h\ 55^m\ 16^s{\cdot}8$; from this date to March 1 it was only $9^h\ 55^m\ 5^s \pm$, and from March 1 to the last observation on June 10, $9^h\ 35^m\ 22^s{\cdot}3$. These values all refer to the marking at the S. Equatorial Belt, and the very quick motion of the intermediate period occurred during the time that the dusky material was passing from the *f.* to the *p.* side of the Red Spot Hollow. It will doubtless be remembered that an analogous acceleration has taken place at former conjunctions of the S. Tropical disturbance with the Red Spot (see *Monthly Notices*, June 1907, pp. 525 and 526, and B.A.A. Report of the Jupiter Section, vol. xvi., part 1, pp. 10 and 19); only, on this last occasion the apparent transference of dark matter across the bay was seemingly very much slower than is usually the case. There is some doubt as to the exact date when it first arrived at the *p.* "shoulder"; and if it was earlier than March 1, the rotation period during the transference would have to be considerably shortened, but there can be very little doubt that the movement was slower than on previous occasions. Still, a study of the material now accumulated clearly shows that at the time of conjunction, whichever end happens to be immediately concerned, the apparent motion of the dark material invariably becomes strikingly accelerated, and that it is during its passage across the bay that the disturbance attains its maximum length.

The Great Red Spot.—Many of the variations that have occurred in the motion of this object during the last five years have been obviously related to the movements of the Great S.

Tropical Disturbance. Thus, at each conjunction the Red Spot has diminished its longitude, and at the end of the conjunction of 1906 the displacement amounted to about 15°. After conjunction, the Red Spot appears to drift slowly back again, and in May 1907 the longitude was rather over 24°. A fresh acceleration then apparently set in, as, when the planet reappeared in the morning sky, early observations made at the end of September and beginning of October 1907 gave a longitude of about 20° or rather less. But the acceleration was only of a temporary character, and the longitude again gradually increased to about 24°·5 in the following January and February, when, for the fourth time, the Great S. Tropical Disturbance came into conjunction with it. Somewhat tardily the Red Spot then began to swing back, and when the observations ceased at the beginning of June the longitude was apparently rather less than 20°.

White Spots in middle of S. Equatorial Belt.—A large number of these objects made their appearance during the apparition as swellings of the white rift which divides the belt longitudinally. But they exhibited many changes, and were not easy to follow and identify. Only three have been considered sufficiently well observed to be selected, and of these, two had a period of about $9^h\ 55^m\ 27^s{\cdot}5 \pm$, and the other—a very prominent object near the *p.* "shoulder" of the Red Spot bay—a period of $9^h\ 55^m\ 46^s{\cdot}1$.

White and Dark Spots at and near N. edge of S. Equatorial Belt and on Equatorial Zone.—As usual, the equatorial spots exhibited some marked irregularities of motion, and those in longitudes 30° ± to 180° ± (System I.) were drifting a good deal more rapidly than those elsewhere during the period covered by the observations. The mean rate, however, has differed but little during the past few years.

White and Dark Spots at S. edge of N. Equatorial Belt.—These features were not nearly so conspicuous as during the previous apparition, and my observations of them are by no means so numerous or so satisfactory as I could wish. In some instances the identifications may be in error, but the period given in the table is *probably* not far from the truth.

Spots in and near N. edge of N. Equatorial Belt.—As already mentioned, some of the objects in the N. Equatorial Belt underwent rapid changes, which it was very difficult or impossible to follow. But all doubtful cases have been discarded, and those selected have been thoroughly well observed. The period seems a good deal shorter than that usually exhibited by N. Tropical spots, and the best observed and most prominent of all—a dark well-defined object at the S. edge of the blue N. component of the belt, in λ 111° on the date of opposition—moved uniformly throughout, with a rotation period of only $9^h\ 55^m\ 17^s{\cdot}6$.

Spots on N.N. Temperate Belt.—These spots have recently shown a decided acceleration. They usually have a period which is considerably longer than that of the adopted zero-meridian of System II. ($9^h\ 55^m\ 40^s{\cdot}632$), and I can only recollect one object

—a dark spot which in 1899 was followed for several months with a mean period of $9^h\ 55^m\ 32^s \pm$ —with a rate similar to those of some of the spots lately observed. The identifications appear to be correct.

Spots in N. Polar shading.—As is the case with the spots in the S. Polar shading, a good deal of difficulty attends the observation of markings on this part of the disc. The positions are not very accordant, and the result given in the table cannot be regarded as having much weight.

(3) *Satellite Phenomena.*

Only three phenomena of special interest were observed, viz. occultations of the first satellite by the second on three consecutive Saturdays. The following are the details of the observations :—

1908 *March* 14.

1st contact $8^h\ 41^m$.
Quite round $8^h\ 42^m{\cdot}7$ to $8^h\ 44^m$.
Last contact $8^h\ 45^m{\cdot}3$.

Definition bad, so times were somewhat uncertain, but no doubt about the completeness of the occultation. Power used 298.

1908 *March* 21.

In contact when first noticed at $10^h\ 50^m{\cdot}5$.
Quite round $10^h\ 52^m$.
Elongated $10^h\ 53^m$.
Last contact $10^h\ 55^m$.

Definition bad. Power 298.

1908 *March* 28.

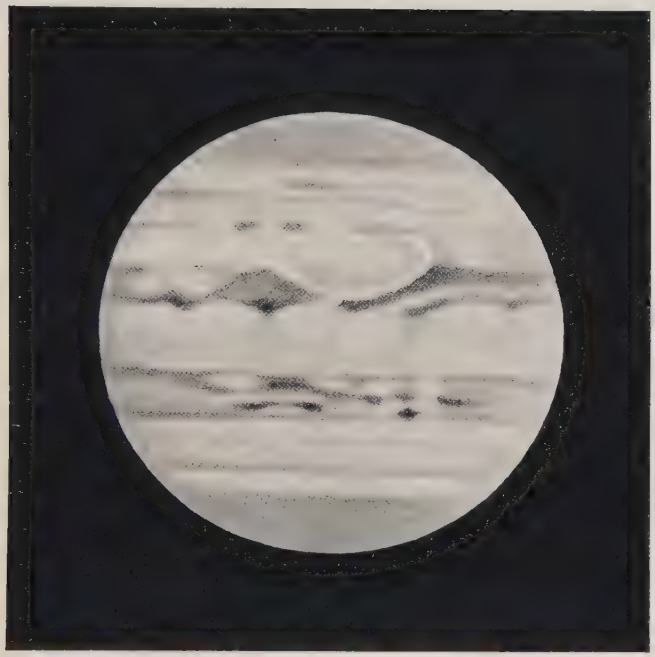
1st contact $12^h\ 0^m$.
Quite round from $12^h\ 3^m$ to $12^h\ 4^m{\cdot}5$.
Last contact $12^h\ 7^m$.

Planet getting low, and definition very bad. Impossible to get anything like a sharp image, and could not use a higher power than 232. Times of contact probably erroneous owing to bad seeing.

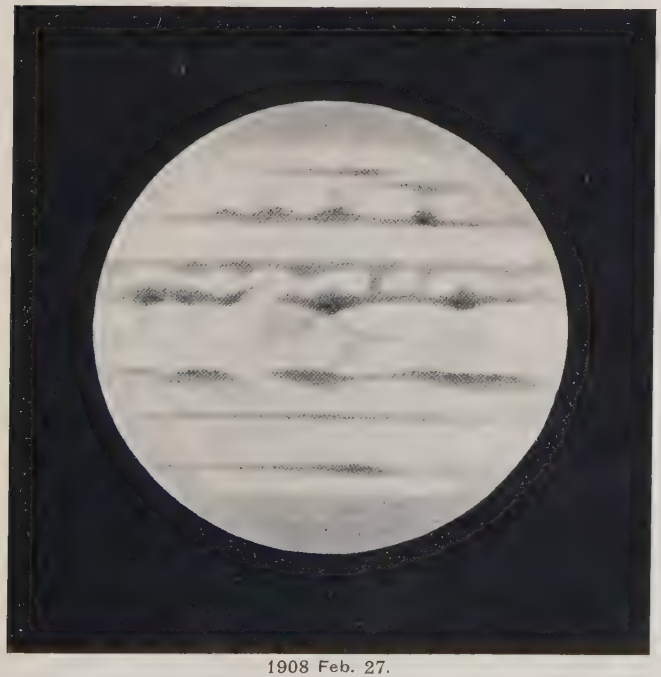
Miscellaneous Notes.

On 1908 *Feb.* 10, between $6^h\ 25^m$ and $16^h\ 35^m$, I observed for the first time rather more than a complete rotation of the planet. The *f.* "shoulder" of the Red Spot bay was seen in transit across the central meridian twice, and altogether about ninety transits of various markings were secured, and sundry other notes and drawings made. Definition, however, which had been good at first, became very bad towards the close of the observations.

On Feb. 27, Jupiter occulted a star, which is probably B.D. + 19°, 2044, mag. 9·4. When observations were begun at about



1908 Jan. 11.
$\lambda = 50^\circ \pm$ System I.: $17 \pm$ System II.



1908 Feb. 27.
$\lambda = 302^\circ \pm$ System I.: $270^\circ \pm$ System II.

DRAWINGS OF JUPITER.—REV. T. E. R. PHILLIPS.

6^h 30^m the star was some two or three diameters of the planet distant. By the time occultation approached the air was very clear, but definition had gone to pieces, and Jupiter's limb was boiling badly.

At 14^h 20^m the star had become exceedingly faint and could scarcely be held steadily, owing to the planet's glare and the atmospheric tremors.

At 14^h 21^m 15^s it was last seen with certainty, when it was separated from the limb by a distance equal to about half that between the S. edge of the S. Equatorial Belt and the S. Temperate Belt.

I thought I glimpsed the star again at 14^h 22^m 30^s, but this was very uncertain. I considered the probable time of occultation 14^h $27^m \pm$, and the point of disappearance the S. Tropical Zone, and near the N. edge of the S. Temperate Belt.

Somewhat curiously, unless my memory is at fault, this is the first time I have observed the occultation of a star by a planet.

Note on the appearance of Saturn's Rings, 1908 *October.*

(*Communicated by the Astronomer Royal.*)

In consequence of an announcement from Geneva Observatory received on October 10, that a new dusky ring had been discovered outside the white rings of Saturn, the ring system has been examined with the 28-inch refractor. The following are the observational notes.

October 10.—Mr. Bowyer observed from 10^h 30^m to 11^h 50^m G.M.T. with powers 450 and 550. Definition fairly good; bright moonlight. The ring, all round, appeared to be dusky on the outer edge. This duskiness was also noticeable on the following limb of the planet.

Mr. Lewis observed from 11^h 45^m to 11^h 55^m G.M.T. The crape ring was then very plain, and Cassini's division well marked. Enceladus and Tethys were very distinct, close to the rings. He remarks: "Two features seem different from what I remember of Saturn in 1895.

"1. The north preceding edge of the outer ring had a hazy border.

"2. The north following edge is fairly sharp and dark,—the impression being that it is the thickness of the ring one is looking at.

"The south edge, preceding and following, is quite normal. The varying effects may possibly be due to the bright moonlight.

Two white dots on the rings near the position of Encke's division, one on each ansa, are very bright."

October 11.—Mr. Lewis observed from $10^h\ 10^m$ to $11^h\ 20^m$ G.M.T. At $10^h\ 10^m$ G.M.T., with a power of 550, three satellites, Rhea, Dione, and Tethys, were seen very distinctly near the rings. There was no sign of the appearances seen on October 10.

At $10^h\ 55^m$ G.M.T. the white spot on the preceding ansa became visible.

At $11^h\ 16^m$ G.M.T., while watching Tethys approach the following limb of Saturn, north of the rings, the edge of the ring just above the satellite appeared indented, while a faint fringe extended hence to the end of the ansa.

At $11^h\ 20^m$ G.M.T. this fringe was seen both preceding and following. Mr. Bowyer observed from $11^h\ 10^m$ to $11^h\ 35^m$ G.M.T. The duskiness on the limb of planet, noticed on October 10, was not apparent. The bright patch near the position of Encke's division was visible on the preceding ansa. At $11^h\ 15^m$ G.M.T. the north following edge had a different appearance from the south edge; there was a trace of a faint, fuzzy ring.

At $11^h\ 25^m$ the faint, fuzzy ring was suspected on the north preceding edge; the appearance of the rings did not change up to $11^h\ 35^m$ G.M.T.

October 12.—Mr. Lewis observed from $10^h\ 20^m$ to $10^h\ 30^m$. The white marks on the rings were not visible. Enceladus and Tethys were seen near the rings. A fringe to the north preceding edge of the outer bright ring was suspected.

Mr. Bowyer observed from $10^h\ 40^m$ to $10^h\ 50^m$ G.M.T. The crape ring was well seen; the white patch was more noticeable on the preceding than on the following side.

At $10^h\ 38^m$ the north edge of outer ring was bordered by a faint, dusky ring, which was seen again at $10^h\ 50^m$.

October 15.—Mr. Bowyer observed from $10^h\ 10^m$ to $11^h\ 15^m$ G.M.T. The crape ring, Cassini's division, and the white patches were seen. The preceding and following edges of the outer ring on the north side seemed sharper than on October 12, and in the short spells of good definition it appeared bordered by an outer dusky ring. At $11^h\ 10^m$, with a power of 670, the outer bright ring had a dullish edge on the north side, while on the south side it was bright. At moments of good definition the north edge was bordered by a dusky ring, fairly well outlined.

Mr. Eddington observed from $10^h\ 45^m$ to $11^h\ 10^m$ G.M.T., power 670. "At the moments of good definition there appeared to be a narrow dusky ring surrounding the bright ring, visible on the N. edge. I could not detect it on the S. edge, nor where it crossed the disk of the planet. The north and south edges differ in colour (owing to atmospheric dispersion), and this renders a comparison of their appearances somewhat uncertain."

The observations on the following evenings were made by Mr. Bowyer.

October 22.—From $10^h\ 15^m$ to $11^h\ 20^m$ G.M.T. At $10^h\ 45^m$

the definition improved so that the white patches could be seen on the rings: with a power of 550 in favourable seeing, there appeared traces of an outer dusky ring on the north preceding and following edge of the white ring.

At $11^h\ 10^m$ the difference in the appearance of the north and south edges was most marked.

October 27.—From $9^h\ 40^m$ to $10^h\ 45^m$ G.M.T. While making micrometer measures of the system, the definition was occasionally very fair, and the outline of an outer dusky ring could be seen on the north preceding and following edge of the white ring.

October 30.—From $9^h\ 30^m$ to $10^h\ 45^m$ G.M.T. Under similar circumstances and definition, the two bright patches were seen, and an outer dusky ring was seen bordering the north preceding and following edge of the outer bright ring.

October 11.—On this evening two interesting observations were made by Mr. Lewis. Incidentally, they show the correctness of the adjustment to focus. The two satellites Dione and Rhea were very close together near the south following edge of ring. The position angle and distance were measured.

	h	m	s	
Angle of position, Dione to Rhea, 356°·1 at	0	22	48	Greenwich sid. time
Distance from Dione to Rhea, 2″·18 at	0	23	13	,, ,,

A transit of Tethys was also observed.

	h	m	s	
First contact of Tethys and Saturn,	0	49	24	Greenwich sid. time
Last contact of Tethys and Saturn,	0	52	1	,, ,,

Royal Observatory, Greenwich:
1908 *November* 9.

Observations of Minor Planets, from photographs taken with the 30-in. Reflector of the Thompson Equatorial at the Royal Observatory, Greenwich, during the year 1907.

(*Communicated by the Astronomer Royal.*)

The following positions of minor planets were obtained from photographs taken with the 30-in. reflector during the year 1907.

The plates were measured with the astrographic micrometer. Six reference stars were, as a rule, measured with the planet, their positions being derived, when possible, from the Catalogues of the Astronomische Gesellschaft, or from the Radcliffe Catalogue 1890.

The positions given are not corrected for Parallax.

Log Parallax Correction = log Parallax Factor − log Δ.

Date and G.M.T. 1907. d h m s	Apparent R.A. h m s	Apparent Dec. ° ′ ″	Log. Parallax Factor. R.A.	Dec.
	(8) Flora.			
Jan. 30 9 7 10	7 18 44·22	+23 36 22·6	−9·211	+0·631
Feb. 1 11 16 26	7 16 59·00	23 46 15·2	+8·873	0·612
	(402) Chloë.			
Feb. 1 11 29 52	8 47 41·17	+18 18 36·1	−8·756	+0·681
21 12 0 44	8 31 35·86	21 27 3·6	+9·187	0·656
Mar. 1 11 45 41	8 27 33·68	22 25 2·1	+9·270	0·652
	(511) Davida.			
Feb. 1 11 45 33	10 5 8·63	+25 24 20·7	−9·212	+0·608
21 11 39 55	9 49 24·84	28 15 19·5	−8·037	0·536
Mar. 1 11 55 46	9 43 24·31	29 3 7·1	+8·931	0·529
	(11) Parthenope.			
Mar. 20 10 41 30	10 41 6·06	+13 17 5·4	−8·215	+0·733
21 11 32 8	10 40 18·23	13 22 19·0	+8·866	0·734
22 10 41 52	10 39 34·84	13 27 2·4	−7·517	0·732
23 10 43 11	10 38 50·83	13 31 44·1	+7·915	0·731
	(79) Eurynome.			
Mar. 20 11 0 29	11 24 14·25	−0 21 16·2	−8·733	+0·838
21 11 43 28	11 23 21·17	0 13 42·7	+8·317	0·837
22 10 59 11	11 22 31·87	−0 6 37·7	−8·618	0·837
23 10 57 6	11 21 41·66	+0 0 37·1	−8·583	0·836

Date and G.M.T. 1907. d h m s	Apparent R.A. h m s	Apparent Dec. ° ′ ″	Log. Parallax Factor. R.A.	Dec.
	(82) Alkmene.			
Mar. 20 11 19 35	11 52 56·03	+3 24 43·6	−8·845	+0·814
21 11 54 24	11 52 3·14	3 28 42·7	−7·806	0·813
22 11 16 37	11 51 13·30	3 32 26·0	−8·771	0·813
23 11 13 52	11 50 22·41	3 36 10·8	−8·747	0·813
	(68) Leto.			
Mar. 22 11 30 38	12 42 26·49	+3 12 31·8	−9·072	+0·816
23 11 27 34	12 41 37·65	3 16 39·1	−9·060	0·816
	(488) Kreusa.			
Mar. 23 11 44 11	12 19 36·66	+17 48 11·4	−8·755	+0·687
Apr. 6 11 42 29	12 8 59·87	18 24 43·0	+8·702	0·680
18 10 13 2	12 1 40·33	18 19 45·4	−7·925	0·679
22 10 10 29	11 59 48·09	18 10 44·0	+8·226	0·681
	(588) Achilles.			
Apr. 6 11 0 1	11 22 31·77	−5 10 57·0	+8·735	+0·864
May 4 9 44 36	11 14 29·73	4 9 40·1	+9·077	0·858
	(35) Leukothea.			
Apr. 22 10 53 22	12 51 42·80	−12 43 10·4	+7·214	+0·897
24 10 19 57	12 50 12·49	12 40 17·8	−8·575	0·897
May 2 10 36 52	12 44 54·06	12 29 30·2	+8·704	0·896
	(57) Mnemosyne.			
Apr. 22 10 31 44	12 57 34·77	−6 5 6·3	−8·636	+0·869
24 10 7 17	12 56 23·76	5 50 55·4	−8·627	0·867
	(31) Euphrosyne.			
May 7 10 14 27	13 29 34·04	−8 20 19·4	−8·438	+0·879
8 10 53 0	13 28 42·91	8 21 42·7	+8·639	0·879
	(451) Patientia.			
Apr. 18 10 37 3	14 24 31·69	+5 41 16·0	−9·278	+0·803
24 10 35 52	14 19 46·03	5 55 58·6	−9·180	0·799
May 2 10 53 25	14 13 21·86	6 7 35·4	−8·816	0·795
	(6) Hebe.			
May 7 13 6 1	14 49 26·74	+7 10 26·6	+9·074	+0·789
8 11 20 56	14 48 36·53	7 14 6·3	−8·604	0·786

Date and G.M.T. 1907. d h m s	Apparent R.A. h m s	Apparent Dec. ° ′ ″	Log. Parallax Factor. R.A.	Dec.
(393) Lampetia.				
May 2 11 10 9	14 56 38·16	−12 18 56·2	−9·040	+0·893
7 12 46 25	14 52 31·48	11 16 30·1	+8·932	0·890
8 11 7 58	14 51 45·50	11 4 58·3	−8·822	0·890
(9) Metis.				
May 7 13 21 59	14 53 55·94	−12 52 25·5	+9·144	+0·893
8 11 32 57	14 52 59·70	12 50 8·0	−8·445	0·898
(86) Semele.				
May 18 10 16 38	14 59 18·78	−12 42 32·6	−8·995	+0·895
19 12 18 33	14 58 30·22	12 40 1·2	+9·028	0·895
(199) Byblis.				
May 2 11 30 38	15 37 26·93	−2 32 41·9	−9·138	+0·850
7 11 38 19	15 33 23·49	2 34 7·4	−8·948	0·850
8 11 50 51	15 32 32·61	2 34 49·2	−8·793	0·851
(485) Genua.				
June 19 11 44 2	17 7 31·35	−4 36 20·8	+8·602	+0·861
July 4 10 54 1	16 56 37·06	4 35 36·2	+8·855	0·861
6 10 56 16	16 55 23·23	4 37 42·4	+8·949	0·861
(17) Thetis.				
June 19 12 16 21	18 27 28·92	−17 32 2·1	+8·585	+0·913
24 12 33 52	18 22 52·19	17 48 59·3	+8·503	0·914
July 4 11 26 29	18 13 35·07	18 27 2·8	[0]	0·916
(190) Ismene.				
June 19 12 52 48	18 42 56·85	−15 22 4·9	−7·520	+0·907
(148) Gallia.				
July 10 11 22 11	20 20 12·00	−1 21 37·5	−9·217	+0·843
15 12 45 7	20 16 21·93	2 1 49·6	[0]	0·848
Aug. 6 12 39 57	19 58 5·21	5 55 37·3	+9·188	0·865
(65) Cybele.				
Aug. 6 13 7 42	20 44 5·03	−15 37 54·2	+9·119	+0·902
19 11 10 41	20 35 21·16	16 24 43·2	+8·601	0·910
(185) Eunice.				
Aug. 11 12 2 56	21 15 31·53	−4 58 45·1	+7·904	+0·864
15 11 31 36	21 12 29·44	5 53 25·7	−8·109	0·868
19 10 37 26	21 9 31·42	6 49 21·4	−8·847	0·872

Date and G.M.T. 1907. d h m s	Apparent R.A. h m s	Apparent Dec. ° ′ ″	Log. Parallax Factor. R.A.	Dec.
	(431) Nephele.			
Aug. 6 13 41 44	22 8 42·05	−12 50 35·3	+8·704	+0·897
10 11 25 12	22 6 6·60	13 8 27·8	−9·149	0·894
11 11 7 22	22 5 25·48	13 13 5·2	−9·205	0·893
15 11 23 38	22 2 33·49	13 32 7·3	−9·029	0·898
28 10 48 1	21 53 0·03	14 32 16·3	−8·828	0·903
	(44) Nysa.			
Aug. 6 13 41 44	22 9 46·27	−12 42 30·1	+8·690	+0·897
10 11 25 12	22 6 32·92	13 5 58·9	−9·149	0·894
11 11 7 22	22 5 42·08	13 12 2·9	−9·205	0·893
15 11 23 38	20 2 9·88	13 36 56·8	−9·029	0·898
28 10 48 1	21 50 19·54	14 56 16·0	−8·796	0·904
	(117) Lomia.			
Sept. 24 9 52 20	22 21 1·12	−12 26 8·7	−8·469	+0·896
26 9 32 24	22 19 36·62	12 18 20·9	−8·675	0·895
	(516) Amherstia.			
Sept. 23 9 52 20	22 41 58·09	− 1 56 39·5	−8·835	+0·847
24 9 35 12	22 41 10·22	1 58 49·7	−8·948	0·847
26 9 50 28	22 39 35·97	2 3 11·4	−8·694	0·848
	(71) Niobe.			
Sept. 23 10 28 39	22 59 25·28	+17 50 24·4	−8·604	+0·686
26 10 9 28	22 56 38·80	17 41 41·9	−8·686	0·688
	(122) Gerda.			
Sept. 23 10 47 51	23 11 1·60	− 4 54 39·1	−8·435	+0·863
24 10 24 40	23 10 23·02	4 59 8·0	−8·748	0·863
	(554) Peraga.			
Sept. 23 11 4 48	23 17 28·27	+ 0 26 37·1	−7·982	+0·833
24 10 45 22	23 16 35·02	0 21 33·5	−8·525	0·834
26 10 26 5	23 14 49·70	0 11 19·0	−8·679	0·835

Date and G.M.T. 1907. d h m s	Apparent R.A. h m s	Apparent Dec. ° ′ ″	Log. Parallax Factor. R.A.	Dec.
	(433) Eros.			
Sept. 23 11 19 36	1 0 31·03	+35 6 18·0	−9·252	+0·433
24 10 56 42	0 59 10·00	35 19 55·8	−9·324	0·445
26 10 41 42	0 56 15·82	35 46 3·7	−9·340	0·440
Oct. 12 12 13 30	0 26 6·96	37 44 15·8	+9·138	0·344
15 9 29 39	0 20 6·05	37 44 50·1	−9·184	0·351
19 8 14 11	0 12 0·96	37 34 59·3	−9·392	0·415
22 9 4 5	0 6 4·19	37 19 28·9	−9·083	0·350
25 9 12 39	0 0 32·93	36 57 47·0	−8·843	0·345
	(326) Tamara.			
Oct. 15 11 22 11	1 16 28·32	− 2 41 0·1	−9·103	+0·850
25 10 39 41	1 3 14·69	1 56 19·7	−8·245	0·847
	(441) Bathilde.			
Oct. 12 13 4 40	1 31 4·12	+19 56 38·7	+8·967	+0·665
	(53) Calypso.			
Oct. 12 12 31 1	2 18 28·70	+5 27 10·0	−8·619	+0·800
	(563) Suleika.			
Nov. 15 10 50 40	2 39 12·96	+2 27 8·5	−8·318	+0·820
29 11 9 46	2 28 26·57	3 30 8·3	+9·054	0·814
Dec. 5 8 47 0	2 25 22·88	4 8 20·9	−8·845	0·809
6 8 47 31	2 24 58·04	4 15 26·1	−8·794	0·809
	(192) Nausicaa.			
Nov. 15 12 2 45	4 6 4·77	+34 22 37·7	−8·733	+0·411
Dec. 10 11 26 52	3 38 56·32	32 56 59·6	+9·067	0·458
	(617) Patroclus.			
Nov. 29 9 59 59	4 39 40·73	+35 54 55·8	−9·383	+0·451
29 10 37 13	4 39 39·60	35 54 57·5	−9·247	0·414
Dec. 3 9 33 12	4 36 48·22	35 59 7·1	−9·406	0·459
10 12 47 2	4 31 39·85	36 3 32·6	+9·239	0·407

Royal Observatory, Greenwich:
1908 *November* 4.

Photographs of Comet c 1908, *taken at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

This comet has been under observation at the Royal Observatory since the announcement of its discovery by Morehouse at the Yerkes Observatory.

Photographs with short exposure, for determination of its position, have been taken with the 30-inch reflector on every available night, but in addition an extensive series of photographs with long exposures, varying from 5 to 60 minutes, have been secured on every possible occasion. The first photograph, taken on September 4, showed that the comet had a considerable tail. Subsequent photographs on September 6, 7, and 8 showed such remarkable changes in the formation of the tail that it was obvious that it would be necessary to photograph at much shorter intervals than 24 hours in order to follow these changes. It was decided, therefore, that photographs should be taken at short intervals through the night. At first exposures up to about 60 minutes were given, sufficient to show the tail extending to the edge of the plate, a distance of about 90′, but later, as the comet became brighter, the exposure was shortened to 10–15 minutes so that the fine detail should not be blurred by the motion of the particles in the tail.

Supplementing the above series, a number of photographs have been obtained with a portrait lens of $3\frac{1}{4}$ inches aperture and 11 inches focal length (kindly lent by Mr. W. Bowyer), with exposures ranging from 24 minutes to 90 minutes.

List of Photographs of Comet c 1908, *taken with the 30-inch Reflector.*

Date. 1908.	Limiting Times of Photographs (Middle of Exposure), G.M.T.	No. of Photos.	Exposure.	Remarks.
	h m h m		m m	
Sept. 4	15 44	1	10	Guiding imperfect, the motion of the comet being unknown. Tail extends 30′.
6	14 14–15 31	2	30, 47	Principal feature — parabolic envelopes, which are unsymmetrically arranged about nucleus.
7	14 56–15 48	2	60, 15	Coma bright; envelopes no longer pronounced; tail consists of wavy streamers much disturbed. Dark rays shown. These appear to be darker than the background of the sky.
8	15 37	1	45	Head similar to Sept. 7; tail consists of two main streamers much contorted.

Photographs taken with the 30-*inch reflector*—continued.

Date. 1908.	Limiting Times of Photographs (Middle of Exposure), G.M.T.	No. of Photos.	Exposure.	Remarks.
	h m h m		m m	
Sept. 14	7 55– 8 22	2	20, 20	Coma condensed. Tail consists of two main rays enclosing an angle of about 9°. A detached V-shaped portion about 20′ from head has moved 1′ in the interval (hourly motion about 2′ away from the head).
17	8 12–10 4	3	45	Bright condensed coma. Tail consists of straight rays. A bright parabolic envelope on the first photograph is shown as straight rays on the last. Tail fan-shaped, with about eight rays.
18	10 0–11 2	2	45	Coma becoming more diffused. Envelopes appear to be forming about nucleus; three shown on second photograph.
21	14 41	1	55	Tail fan-shaped; somewhat similar to Sept. 18.
25	9 21	1	18	Stellar nucleus with several parabolic envelopes. Tail fan-shaped.
26	8 10	1	60	Tail fan-shaped; considerably disturbed.
29	7 39–16 15	9	15 to 50	Stellar nucleus, bright coma with envelopes forming towards end of night. Five shown on last plate. They form at about 2′ from the nucleus (on the side towards the sun). Very bright tail, consisting of wavy streamers considerably disturbed. A pronounced dark ray shown. From measures of features in the tail the following motions have been deduced:— Distance from nucleus, 6′. Motion outwards, 0′·24 per hour. Distance from nucleus, 12′. Motion outwards, 0′·64 per hour. Distance from nucleus, 19′. Motion outwards, 0′·94 per hour.
30	8 8–14 27	6	15 to 61	Very bright tail, more violently disturbed than on Sept. 29. Several pronounced dark rays. From measures of features in the tail:— Distance from nucleus, 5′. Motion outwards, 1′·5 per hour. Distance from nucleus, 6′. Motion outwards, 1′·5 per hour. Distance from nucleus, 8′. Motion outwards, 1′·3 per hour.

Photographs taken with the 30-inch reflector—continued.

Date 1908.		Limiting Times of Photographs (Middle of Exposure), G.M.T.	No. of Photos.	Exposure.	Remarks.
		h m h m		m m	
Oct.	1	7 53–14 35	8	15 to 40	Tail now consists of straight streamers. The outburst has subsided, and the disturbance of Sept. 30 is now seen as a detached mass about a degree away. The motion outward of this mass at a distance of 75′ is 6′·6 per hour.
	2	7 46–10 21	4	20 to 40	Large coma, many envelopes forming. Tail becoming fan-shaped, with several straight streamers.
	3	7 16–13 13	9	15 to 40	Nucleus stellar, with parabolic envelopes. Tail fan-shaped, composed mainly of four strong rays. The fan covers an angle of 60° at commencement of night, but closes into one of 30° at end. Much fine detail.
	5	13 15–16 41	6	5 to 40	Streaming tail, with much fine detail. At distance 4′, motion 0′·8 per hour. At distance 8′, motion 1′·1 per hour. At distance 21′, motion 2′·2 per hour.
	6	7 36– 8 36	2	15, 30	Straight rays from the head; the activity of Oct. 5 has apparently ceased.
	14	8 30–10 14	4	10	Stellar nucleus. Streaming tail, with much fine detail.
	15	7 3–10 25	5	10	Coma brighter. The activity of Oct. 14 has subsided, leaving two principal straight streamers. The tail of Oct. 14 is now seen as a detached mass about 30′ away. At distance 25′, motion 2′·6 per hour.
	18	7 58– 9 50	3	10	Coma bright. Envelopes developing. Tail greatly disturbed, with much fine detail near head. At distance 4′, motion 0′·5 per hour. At distance 21′, motion 1′·4 per hour.
	19	11 56	1	10	Poor sky, little shown.

Photographs taken with the 30-*inch reflector*—continued.

Date 1908.	Limiting Times of Photographs (Middle of Exposure), G.M.T.	No. of Photos.	Exposure.	Remarks.
	h m h m		m m	
Oct. 21	6 26–12 13	8	10 to 15	Stellar nucleus, with many envelopes. Streaming tail much disturbed. At distance 3′, motion 0″·3 per hour. At distance 21′, motion 1″·4 per hour. At distance 34′, motion 2″·4 per hour.
23	7 8–10 47	7	5 to 15	Bright stellar nucleus; many envelopes, developing into finely pencilled straight streamers.
24	6 54	1	6	Stellar nucleus. Streaming tail.
26	9 44	1	10	Very poor sky, little shown.
27	6 41–10 28	10	10 to 20	Many envelopes forming. Tail much disturbed; apparently composed of straight and wavy streamers intermingled. An envelope was observed to form with its apex distant 2′·0 from the nucleus; in 2¼ hours, it had closed in to a distance of 1′·1 from the nucleus.
29	6 38– 8 46	3	20	Stellar nucleus, coma faint. Tail emerges in straight rays, covering an angle of 13°.
30	6 21– 9 19	9	10 to 21	Coma brighter, with many envelopes. Tail composed of wavy streamers much disturbed. A disruption appears about 16′ from head. At distance 16′, motion 2″·2 per hour.
Nov. 3	6 0– 7 54	6	10	Bright coma. Tail composed of wavy streamers, with some disturbance.
8	5 59– 7 24	5	5 to 10	Diffused nucleus. Tail composed of straight rays, with some disturbance.
9	6 5– 6 22	2	10	Wavy, well-defined streamers, curiously bent near the head.
10	5 44– 6 30	4	10	Many envelopes. Tail composed of wavy streamers somewhat disturbed. Some dark rays. At distance 75′ motion 2″·4 per hour.
12	6 38	1	14	Stellar nucleus, many envelopes.

List of Photographs of Comet c 1908, taken with the Portrait Lens.

Date 1908.	Limiting Times of Photographs (Middle of Exposure), G.M.T.	No. of Photos.	Exposure.	Remarks.
	h m h m		m m	
Sept. 20	11 24	1	24½	Tail can be traced 2°. Sharp bend about 10′ from head.
21	13 10	1	40	Tail can be traced 3° to 4°. Broken about 40′ from head.
29	8 12	1	80½	Tail can be traced about 8°.
30	8 38	1	81	Tail can be traced about 6°. Contracts about 1½° from head.
Oct. 1	8 55	1	80	Remarkable photograph. Up to 1° from nucleus, tail consists of fine straight rays. From that point outwards is a bright detached mass, apparently the matter forming the tail of Sept. 30. At 4° from the head the tail becomes much spread out and diffused.
2	9 0	1	75	Tail can be traced 8°. Near the head it consists of fine radiating streamers. At 3° from head commences the diffused mass shown detached on Oct 1. Deduced motion about 5′ per hour.
3	8 50–12 51	2	60, 53	Brightest streamer can be traced 4°.
15	7 4–10 6	2	29½, 33	Tail spreads at a distance of 5°, and can be traced to 7°. Curious break at ½° from head.
18	9 53	1	32	Straight diffused tail 5° long. From 2° to 4° it is faint and broken. At 4° there is a brighter portion.
21	7 22	1	90	Straight tail, which can be traced 7°.
23	9 41	1	40	Tail can be traced 6°.
27	7 31	1	90	Tail can be traced 9°. Very bright at 3½° from nucleus.
28	7 23	1	46	Activity of Oct. 27 has died away. Tail can be traced 7°. Near the head it is composed of fine rays. Broadens suddenly at ½° from head.
29	7 3	1	65	Tail can be traced 8°.
30	6 56	1	40	Very bright tail, which can be traced 11°.
Nov. 3	7 7	1	30	Moonlight. Tail can be traced 5°.
10	5 57	1	30	Tail can be traced 7°.

A study of the photographs shows that the form of the comet's tail exhibits a recurring series of phases, and that the phase seems to bear a relation to the condition of the nucleus which passes through cycles of stages of alternating activity and quiescence.

At the stage of quiescence the comet presents the appearance of a nucleus with little enveloping coma, and straight streamers passing away at little inclination to the general direction of the tail. The coma then grows, envelopes form about the nucleus (on the side towards the Sun), and the rays of the tail become more spread. The stage of maximum activity is now reached, and larger quantities of matter appear to be expelled from the head and then driven back, forming a bright wavy tail, the streamers being no longer straight, but greatly disturbed. This bright tail then appears to be driven off and the stage of quiescence follows.

Much measurable detail is shown on the photographs, and the few tentative measures which have been made (see "Remarks") indicate that the matter forming the tail is moving from the head at an accelerating velocity. At present it is not possible to say more, but when the material has been fully discussed the results will be communicated to the Society.

Royal Observatory, Greenwich:
1908 *November* 9.

On the Photographs of Comet c 1908 (*Morehouse*).
By E. E. Barnard. (Plates 2, 3.)

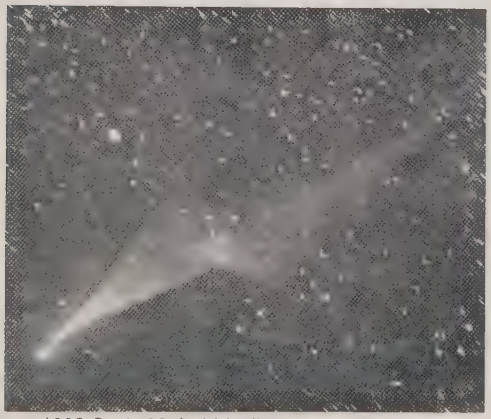
This comet has shown very remarkable phenomena. In some respects they were quite different from those of any comet which has appeared since these bodies have been under photographic study. Even in its quieter phases it has been apparently as active as the larger naked-eye comets. There have, however, been several outbursts that were very extraordinary, and in some respects quite unique. The photographs show that these disturbances occurred on or about September 16, 30, October 1, 6, and 15. I have selected from these photographs the following list of lantern slides which accompany this note. I have separated them into three sets: in the first set the photographs were made with the 10-inch Brashear doublet, the second set with the 6-inch, and the third set (consisting of one plate) with the 3·4-inch doublet, all of the Bruce telescope of the Yerkes Observatory.

List of Lantern Slides of the Comet.

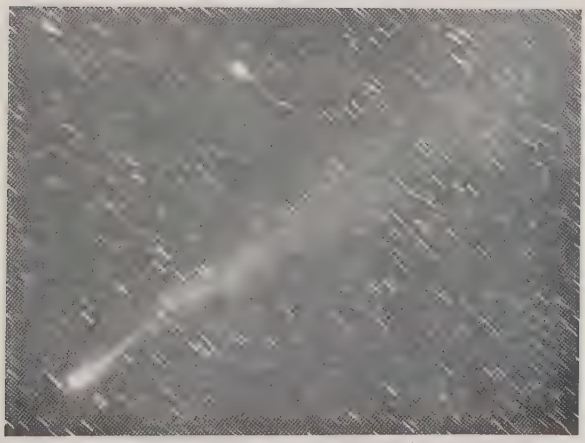
1*st set, made with the* 10*-inch.*

No.	Mid. Exposure C.S.T. 1908.	h m	Duration of Exposure. h m
1	Sept. 30	14 22	1 56
2	Oct. 1	13 43	2 0
3	2	14 59	2 49
4	3	11 7	2 28
5	6	15 58	1 24
6	15	6 57	1 22

1908 Sept. 30 d. 11 h. 16 m. C.S.T. (6-inch.)

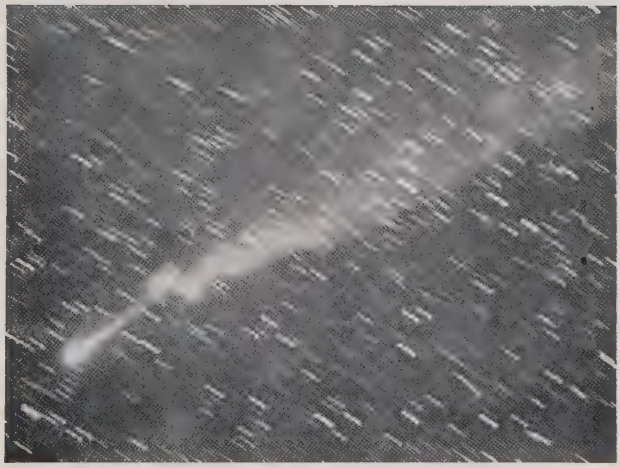


1908 Sept. 30 d. 14 h. 57 m. C.S.T. (6-inch.)



1908 Oct. 6 d. 15 h. 58 m. C.S.T. (10-inch.)

COMET c 1908 MOREHOUSE.—E. E. BARNARD.



1908 Oct. 15 d. 6 h. 57 m. C.S.T. (10-inch.)

1908 Oct. 15 d. 7 h. 34 m. C.S.T. (3·4-inch.)

Comet c 1908 Morehouse.—E. E. Barnard.

2nd set, made with the 6-inch.

	1908.	h m	h m
7	Sept. 30	11 16	2 0
8	30	13 25	2 0
9	30	14 57	0 47
10	Oct. 1	8 45	0 50
11	1	10 48	1 0
12	1	11 55	1 0
13	1	13 15	1 5

3rd set, made with the 3·4-inch.

14	Oct. 15	7 34	2 36

The times are $6^h\ 0^m$ slow of Greenwich.

I understand that an excellent series of photographs of the comet has been obtained at Greenwich. These photographs, if on the same dates as mine, will be of the highest importance in piecing out the history of some of the important changes. It is especially to be hoped that a good set of photographs were secured at Greenwich and elsewhere in Europe on September 30 and October 1, for comparison with pictures taken in other longitudes.

Up to the 22nd of this month I have made photographs of the comet on thirty-seven nights. A large number of these are beautiful pictures, and they are all of great interest.

Of this list, Nos. 1 and 2 are to appear in the *Astrophysical Journal* for November. No. 4 appears in *Popular Astronomy* (along with one on October 4). Nos. 5, 6, 7, 9, 14 are given with this paper (Plates 2 and 3).

Most unfortunately, an interruption has come in our good weather, and it may be that not many more pictures of the comet can be obtained at this observatory.

At present I will not call attention to the peculiarities of these photographs, which, I think, speak for themselves. I must, however, mention the photograph of September 16, which is not represented in these slides. In a very bad sky, in which the comet was scarcely visible in the guiding telescope on account of a very thick, smoke-laden atmosphere, the plates show the tail very long, broad, and violently curved in some parts through almost a right angle to its previous position. On the 17th the tail was again in its normal form and position. From the very poor sky, this picture, though of two hours' exposure, is very weak, and difficult to reproduce.

I must also specially mention the photographs of October 15 On that date I made five photographs each with the three lenses. The first exposure began at $6^h\ 18^m$, and the last one ended at $13^h\ 28^m$. The comet was, therefore, being photographed for over seven hours. After the first two exposures the Moon interfered, and from this cause the last three pictures are not so strong as the first ones. All the features, however, though not so strong, are

well shown. The junction of the narrow tail with the masses changed its position, while the cloud-like masses themselves changed their relative position angles, and apparently were moving faster southward than the comet. At the same time they were receding from it. In a changed form, they are still visible on the photographs of the 16th and 17th. What I wish specially to call attention to are the streamers or secondary tails that issue from these masses. They are specially well shown on slide 14 (with the 3·4-inch lens), reproduced on Plate 3.

It would appear from the light-pressure theory that the large receding masses consist of large and small particles, and that the smaller ones are being driven away from the others with a very much greater velocity by the pressure of the Sun's light. These smaller outgoing particles, therefore, form the streams or secondary tails shown going out from the masses.

Yerkes Observatory :
1908 *October* 24.

Note on Comet c 1908 (*Moorhouse*), 1908 *September* 29–*October* 2.

By the Rev. W. Sidgreaves, S.J., and the
Rev. A. L. Cortie, S.J.

The comet has been photographed on every available night since September 29 at the observatory with a Dallmeyer portrait lens of 6-inch aperture and 30-inch focal length, the generous gift of Mr. Whitelow, F.R.A.S. Pending a full discussion of the photographs, the more remarkable changes in the object between the dates named are set down here.

Plate.	Date of Middle of Exposure. G.M.T.		Length of Exposure. Minutes.	Length of Tail on Plates.
		h m		° ′
1	1908 Sept. 29	9 7·5	45	2 40
2	30	7 45	40	3 2
3	30	9 20	30	3 15
4	Oct. 1	7 42·5	35	3 49
5	1	8 47·5	55	3 27
6	1	11 21	30	2 40
7	2	10 47·5	55	No tail

In the photographs of September 29, 30, the nucleus showed no central condensation, the tail was slightly curved on one side, and possessed a bright central core or axis. This bright central ray

gradually disappeared, the measures of its length being 35′, 23′, and 11′ on plate 1 of September 29 and plates 2–3 of September 30. In plate 3 the nucleus was more stellar in appearance. Accompanying the recession of the bright core, the tail seemed to grow longer. On plate 4 of October 1 the bright axis had disappeared, but at a distance of 1° 1′ measured from the centre of the nucleus was situated a condensation, the tail between the nucleus and condensation being faint, and the secondary tail, of which the condensation was a sort of head, being brighter and wider.

The position of the condensation from the centre of the nucleus was also measured on plates 5 and 6 of the same night with the following result :—

	h	m		°	′	″
At G.M.T. (1)	7	42·5	distance =	1	1	0
(2)	8	47·5	,, =	1	10	0
(3)	11	21	,, =	1	32	40

The times given are the middles of the intervals of exposures. From (1) and (2) the average velocity of recession is 8″·30 per minute. From (2) and (3) the average velocity is 8″·86 per minute. Hence it follows that between the mean epochs of (1) and (2) and (2) and (3) the mean rate of increase of velocity as measured on the plates was ·005″ per minute per minute. The skies having been very hazy, the images on the plates are faint and the measures difficult to make. The images stand no magnification with a lens, so it was only possible to place pointers on them and transfer the distances to a millimetre scale. The measures are therefore only approximate. They seem to show, however, a real acceleration of matter from the head of the comet under a constant force. Also, that the bright condensation measured, was formed as the bright axis disappeared. On October 2 there was no trace of any tail on the plate exposed for 55 minutes. We should have attributed this to the haziness of the sky, had not M. Bigourdan, at the meeting of the Paris Academy of Sciences of October 5 (*Nature*, October 15, 1908), reported that on October 1 the tail had disappeared, and that traces of the tail were noted on October 3. The table given above shows a constant decrease in the length of the tail on the plates of October 1, which we had attributed to the increasing fog and haze in the sky.

Stonyhurst College Observatory:
1908 *November* 9.

Real Paths of Brilliant Meteors observed in 1908.

By W. F. Denning.

The following are the real paths of brilliant meteors observed in England (except No. 3 recorded in Holland and Belgium) and computed by me during the present year. I gave a few details of several fireballs which appeared in 1907 in *The Observatory* for June 1908.

As far as I can judge, a rather unusual number of large meteors have been seen and described during the last two years, but in many cases the accounts have been isolated, so that definite results as to the height, etc. could not be ascertained.

The Perseids were fairly rich in 1908, and included a large proportion of bright objects, several of which were recorded at two stations. At Bristol the first Perseid was noticed on July 21, the last on August 19. The extreme limits of visibility of the shower appear to be from July 7 to August 25, with a radiant moving to E.N.E. from $7^\circ + 44^\circ$ to $66^\circ + 59^\circ$. But minor systems are somewhat involved with the true Perseids, and it is occasionally difficult to separate them from Cassiopeids and Camelopardids in the earlier and later stages of the display.

Date.	G.M.T.	Mag.	Height		Path. Miles.	Velocity per sec. Miles.	Radiant.	
			at first. Miles.	at end. Miles.			α δ	
1908.	h m							
May 17	9 43	$>$ ♀	76	28	57	15	194°+20°	Coma Berenicid
19	10 20	$>$ ♀	69	45	142	22	252 − 20	α Scorpiid
27	12 28	$>$ ♀	58	55	120	15	76+43	α Aurigid
June 25	10 43	$>$ 1	61	59	51	30	43+37	β Perseid
28	11 12	♀	67	45	84	12	237 − 18	η Librid
July 1	9 15	$\frac{1}{2}=$ ☽	61	27	105	18	250 − 20	α Scorpiid
28	11 6	$=$ ☽	82	40	50	34	302+23	η Sagittid
Aug. 2	10 26	$>$ 1	82	54	50	25	39+56	Perseid
2	10 30	♃	77	47	57	38	38+54	Perseid
2	11 2	1	79	55	40	40	34+54	Perseid
2	12 7	♃	61	49	32	21	337 − 11	δ Aquarid
10	10 17	♃	72	45	52	29	43+57	Perseid
11	9 44	$>$ 1	99	52	93	35	46+59	Perseid
11	11 38	1	90	51	61	...	44+57	Perseid
Sept. 14	8 48	2 × ♀	59	32	71	...	6+ 7	γ Pegasid
Oct. 14	9 10	$>$ ♀	70	18	96	24	42+20	ϵ Arietid

Note on the number of Faint Stars with large Proper Motions.
By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. The existence of stars like Groombridge 1830, which is of 6·5 magnitude and has a P.M. of 700″ a century, larger than that of the brightest star known, suggests that there may be stars of (say) the 12th magnitude with proper motions so large as to be easily measurable. At first sight it would also appear that since there are many more stars of the 12th magnitude than of the 6th, the cases of large P.M. would also be more numerous. Accordingly, when Mr. C. L. Brook generously presented a stereocomparator to this observatory, search was made on plates for the Astrographic Catalogue which had, for one reason or another, been repeated after an interval of some years, for cases of sensible proper motion among faint stars. The results were most disappointing, practically nothing being found after a good deal of time spent in searching. It was thought that perhaps the réseaux on the two plates troubled the observer too much, and rendered his eyes less sensitive to small differences (see remarks by Dr. Max Wolf in *Mon. Not.*, lxiv. p. 112), and it was determined to take some plates without a réseau when the work of revision and printing of results for the catalogue was completed. The increase of the interval in the meantime would be a gain.

2. A recent visit of Professor Kapteyn to Oxford recalled attention to the question of P.Ms. of faint stars. He was anxious to get some provisional results, and it occurred to me that it would be worth while to compare the actual measures of plates of the same region taken at an interval of ten to fifteen years. We have many plates which were taken and measured in 1892 and following years, but which do not come up to the standard of the later work, and were accordingly repeated recently in the general revision which Mr. Bellamy has assiduously conducted as we come to print each zone. For stars in the same region, we thus have two sets of measures separated by an interval comparable with that proposed by Professor Kapteyn in his scheme for "sampling" the heavens; and since provisional constants have been deduced for each plate, the work of reducing one to the other is straightforward. It involves, of course, a considerable amount of arithmetical work; but it seemed desirable to face this labour in order to get a general idea of the results to be expected.

3. A number of regions have been already examined, and it soon became clear why we had drawn blank with the stereocomparator. The number of sensible P.Ms. of faint stars is in fact very small, and the results for $+28^\circ$ zone, which is now completed, will illustrate this point.

4. In zone $+28°$ we had the following regions, giving an interval of over ten years.

R.A. of Centre.	Total Stars.	Interval.	Stars examined.	Reviser.	Centennial P.Ms. 15″–20″	Centennial P.Ms. >20″
h m		y				
0 36	182	12·2	6	J.H.W.	0	1
5 51	310	11·9	23	G.H.H.	2	0
7 30	191	12·1	14	H.H.T.	1	0
20 51	431	14·8	30	J.H.W.	1	0
21 0	359	14·8	20	H.H.T.	1	0
21 36	294	14·8	24	H.H.T.	0	2
21 45	264	14·8	11	J.H.W.	3	1
22 21	308	14·8	11	J.H.W.	0	1
22 30	230	14·8	10	G.H.H.	1	1
22 39	253	14·8	11	H.H.T.	1	0
Total	2822		160		10	6

Column 1 is the R.A. of plate centre, the declination being $+28°$.

Column 2 gives the total number of stars measured on the earlier plate, which was, from the nature of the case, deficient in stars, or it would not have been repeated. The earlier plate was reduced to the later by use of the constants obtained independently for each, which can be readily combined to give relative constants.

Column 3 gives the interval in years and tenths between the two exposures to the region.

Column 4 shows the number of cases of differences sufficiently large, either in x or y, to be worth examining specially. As a rule, all differences greater than $\pm 1''{\cdot}3$ in x or y were examined. A large number of them were sensibly reduced on remeasurement, as was only to be expected; for they include all the cases where an extreme error of measurement in one direction on one plate happens to coincide with an extreme error in the opposite direction on the other plate. They also include cases of faulty images in the corners of the plates; of large elongated images of bright stars difficult to measure; and finally, a few (very few, we were glad to find) actual mistakes in the earlier plates,—none in the later plates, since such as existed had probably been caught in the proof-reading.

Column 5 shows the observer who examined the differences of column 4. In this work I am glad to acknowledge gratefully the volunteer assistance of Mr. J. H. Worthington, F.R.A.S., and Mr. G. H. Hamilton. Their remeasures were made with great care, in reversed positions of the plate.

Columns 6 and 7 show the cases of large P.M. detected. There were, of course, a number of smaller P.Ms. than these which are reasonably certain, viz.—

8	of centennial	P.M.	14″·0 to 14″·9
6	,,	,,	13·0 to 13·9
5	,,	,,	12·0 to 12·9
10	,,	,,	11·0 to 11·9
14	,,	,,	10·0 to 10·9

and so on. But these numbers cannot be considered accurate, since they would no doubt be increased by the addition of cases where a coincidence of errors in measurement annulled an existing P.M. Such cases could only be detected by re-measurement of all the stars, and this cannot be undertaken just now. For the moment, we are concerned with definite results only for the P.Ms. outside a certain limit, which has been provisionally fixed as 15″ centennial.

5. Now these large P.Ms. are by no means all of faint stars. They include several already well-known P.Ms., as for instance that of ϵ Andromedæ (Oxford +28° 1540$_*$). The asterisks in the second column of Table II. show that the corresponding stars are sufficiently bright to be in the Cambridge Meridian Catalogue, and it will be seen that there are 7 of these out of the 16. Table II. shows the details.

TABLE II.

Centennial P.M.	Oxford No.	Camb. No.	Δx.	Δy.	Mag.
″	°				
42·2	+28·65996	...	+21	− 2	10·4
31·2	+28·66265$_*$	12835	+10	−12	4·5
29·4	+28·69159	...	+14	+ 5	10·7
24·4	+28·67189$_*$	12951	+10	+ 7	8·9
22·9	+28·69875	...	− 5	−10	10·8
22·8	+28·1540$_*$	370	− 6	− 7	4·1
19·7	+28·61985$_*$	11904	+ 8	− 6	8·0
18·9	+28·66897$_*$	12981	− 8	− 5	6·8
17·1	+28·12579	...	+ 4	− 6	11·4
17·1	+28·66910	...	− 8	− 3	11·5
16·8	+28·69669	...	+ 8	+ 3	11·0
16·5	+28·63432	...	− 2	− 8	11·1
16·2	+28·70256$_*$	13650	+ 8	− 1	9·0
16·1	+28·67027$_*$	12959	− 7	− 4	9·3
15·9	+28·22900	...	+ 6	+ 2	10·9
15·4	+28·12121	...	+ 6	+ 2	11·2

6. Column 1 of Table II. shows the total P.M. per century.

Column 2 gives the Oxford number in vol. iv. of the Oxford Astrographic Catalogue; the asterisk denoting a reference star in the Cambridge A.G. Catalogue for 1875·0, the Cambridge number being given in Column 3.

Columns 4 and 5 show the total measured difference on the two plates in units of ·001 of a réseau space, or 0″·3. The intervals are smaller for some plates than others, which must be remembered in comparing this column with the first.

The last column gives a provisional magnitude, which for the bright stars is the Cambridge magnitude, and for the others is deduced from the formula printed at the head of each plate in the Oxford volume. These magnitudes are uncertain for the fainter stars, and may be a whole magnitude in error perhaps; but this will not affect the main point of the present note.

7. As a contrast with this table, we may take the results of the

new reduction of Groombridge's Catalogue recently published by the Royal Observatory at Greenwich. On p. cxi. of the Introduction is given a list of 9 stars with "extremely great proper motion," and another list of 163 stars with centennial P.M. greater than 20″, making 172 stars in all with P.M. greater than 20″ out of 4243. On the same scale, we should expect out of our 2822 stars, 115 with P.M. exceeding 20″, whereas we only get 6, and only 3 of these are of the faint stars which form the bulk of those examined. The discrepancy is very considerable, and even surprising. If we pick out the stars with P.M. greater than 20″ a century for the different magnitudes we get the following figures :—

Mag.	To 4·9.	5·0 to 5·9.	6·0 to 6·9.	7·0 to 7·9.	8·0 to 8·9.	Total.
No. Stars P.M. > 20″ .	34	35	49	35	19	172
Total Stars in Catalogue	234	538	1152	1374	930	4228
Percentage . . .	14·6	6·51	4·26	2·56	(2·05)	
Log (per cent.) . .	1·16	0·81	0·63	0·41	(0·31)	

The drop in the log (percentage) is about 0·20 per magnitude: the numbers for the 8·0 to 8·9 magnitudes are bracketed, since the search is probably far from complete. Carrying on this drop to magnitudes 11·0 to 11·9, we should get—

	8·0 to 8·9.	9·0 to 9·9.	10·0 to 10·9.	11·0 to 11·9.	Total.
Log (per cent.). . .	0·21	0·01	9·81	9·61	
Percentage . . .	1·62	1·02	0·65	0·41	
Total Stars on Oxford plates	60	216	684	1840	2800
Product	1·0	2·2	4·5	7·5	15·2

The distribution of the stars among the higher magnitudes is only a rough estimate, but cannot affect the expectation seriously. We see that, on the hypothesis that the drop in the log (percentage) is carried on uniformly, we might have expected 15 stars of over 20″ cent. P.M. and of magnitude fainter than 8·0, whereas we only get 4. The drop must therefore be much more rapid unless our figures are quite exceptional.

8. The work is being extended to other zones, and further results will be given at an early date. Meanwhile, the following remark seems worth making. Professor Kapteyn is proposing to determine the average Proper Motions of faint stars from photographs of specified regions taken at an interval of about fifteen years. Will this interval be long enough? The suggestion of the above results (it is, of course, only a suggestion at present) is that the P.Ms. are so small that a much longer interval will be necessary to disentangle them from the unavoidable accidental errors.

Summary.

There seem to be strangely few proper motions of over 20″ a century among the faint stars.

University Observatory, Oxford:
1908 *November* 12.

On the Diminution of Light in its passage through Interstellar Space. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. The question whether light suffers any diminution in its passage to us from distant stars is by no means new, and has been hitherto answered provisionally in the negative. Thus Newcomb writes in his book, *The Stars: a Study of the Universe* (p. 231, edition of 1901):—

"The conclusion that the universe is finite rests upon the hypothesis that light is never lost in its passage to any distance, however great. This hypothesis is in accordance with our modern theories of physics, yet it cannot be regarded as an established fact for all space, even if true for the distances of the visible stars. . . . The hypothesis of a limited universe and no extinction of light, while not absolutely proved, must be regarded as the one to be accepted until further investigation shall prove its unsoundness."

2. Newcomb is here considering loss of light to the ether. But light may be lost in other ways; and it has been found that a very reasonable hypothesis of scattering of light by small particles in space will explain the following facts:—

(*a*) The ratios of the number of stars per magnitude tabulated by Kapteyn as far as the twelfth magnitude.

(*b*) The fact that when photographic exposure is prolonged in a ratio which ought to give stars fainter by *five* magnitudes, we only get *four* visual magnitudes. If there is scattering of light by small particles, it should be greater for photographic than for visual rays, according to Rayleigh's well-known law of λ^{-4}; and the puzzling failure of photographic exposure, which has been attributed provisionally to the behaviour of the plate, is thus explained as a celestial phenomenon.

(*c*) The ratios of the counts of stars for different exposures found at Greenwich, with a range from $20^{\text{sec.}}$ to $40^{\text{min.}}$

(*d*) The well-known fact that the total light of all the stars is comparatively small.

So far as I am aware, these are *all* the available facts of a general nature; and a simple hypothesis which accounts for them all is, therefore, worth attention.

3. First consider the simple case when all the stars are of the same brilliance, B, and are uniformly distributed through space. Let the distance of a star be r, and its apparent magnitude m. Then—

$$10^{-m\times 0\cdot 4} = Br^{-2} \quad . \quad . \quad . \quad . \quad (1)$$

$$\therefore \quad dm \times 0\cdot 4 \log_e 10 = 2\, dr/r \quad . \quad . \quad . \quad (2)$$

Hence the number of stars between magnitudes m and $m+dm$ is, if D be the number in unit volume,

$$4\pi D r^2 dr = 2\pi D \times 0{\cdot}4 \log_e 10. \quad r^3 dm$$

$$= D_1. \quad B^{\frac{3}{2}}. \quad 10^{m\times 0{\cdot}6}. \, dm,$$

where D_1 is D multiplied by a numerical factor. And the total number of stars brighter than magnitude m and fainter than M is

$$N = D_1 B^{\frac{3}{2}} \int_M^m 10^{m\times 0{\cdot}6} \, dm$$

$$= D_2 B^{\frac{3}{2}} \left[10^{m\times 0{\cdot}6} - 10^{M\times 0{\cdot}6} \right]$$

where D_2 is D multiplied by another numerical factor. When M is negative, as it certainly is for the brightest stars, the second term in the bracket is so small that we may neglect it and write

$$\log N = \text{const.} + m. \times 0{\cdot}6.$$

Since $10^{0{\cdot}6} = 3{\cdot}98$, the total number should increase nearly 4 times for each magnitude,—a well-known result, which has often been quoted. The observed ratio is about 3·5, and the defect from 4·0 has been attributed to the fact that the stars are not of uniform brilliancy B, or to other causes.

4. But has it not been hitherto overlooked that if we conceive the existing universe made up of superposed systems of uniform brilliance, however many, the above law of increase in a geometrical progression with ratio 4 will hold for each system, and therefore for the whole? If the variation in B is not dependent on distance from our Sun, we can add up all the coefficients $D_2 B^{\frac{3}{2}}$ and still keep the common ratio. The appropriate expression is the integral

$$10^{m\times 0{\cdot}6} \, dm \times \int B^{\frac{3}{2}} D_2 dB \qquad . \quad . \quad . \quad (4)$$

$$= 10^{m\times 0{\cdot}6} \, dm \times C,$$

so that the law of increase in number with magnitude will be the same as if the stars were all of a certain uniform brilliancy; and the increase per magnitude will still be 4 times if there is no absorption of light. The fact that Kapteyn finds, after a very careful discussion, the factor 3·5 (about) instead of 4 is thus evidence of absorption of light. If we admit the existence of absorption, the above demonstration no longer holds. We shall consider that the stars are distributed according to the law above indicated, and the hypothesis submitted for consideration can now be stated most simply by tabulating the facts as below.

TABLE I.

(1) True Mag.	(2) r.	(3) Log N.	(4) $\cdot 3r$.	(5) Visual Mag.	(6) Kapteyn.	(7) V—K.	(8) $\cdot 6r$.	(9) Photog. Mag.
0·0	0·063	5·50	0·02	0·02	9·64	+0·38	0·04	0·04
1·0	0·10	6·10	·03	1·03	0·68	+0·35	0·06	1·06
2·0	0·16	6·70	·05	2·05	1·76	+0·29	·10	2·10
3·0	0·25	7·30	·08	3·08	2·90	+0·18	·15	3·15
4·0	0·40	7·90	·12	4·12	4·00	+0·12	·24	4·24
5·0	0·63	8·50	·19	5·19	5·16	+0·03	·38	5·38
6·0	1·00	9·10	·30	6·30	6·36	−0·06	·60	6·60
7·0	1·58	9·70	·47	7·47	7·60	−0·13	0·95	7·95
8·0	2·51	0·30	·75	8·75	8·89	−0·14	1·51	9·51
9·0	3·98	0·90	1·20	10·20	10·28	−0·08	2·40	11·40
10·0	6·31	1·50	1·89	11·89	11·78	+0·11	3·79	13·79
11·0	10·00	2·10	3·00	14·00	13·47	+0·53	6·00	17·00

The *first* column gives the theoretical magnitude of the star on the assumption of no absorption of light, on Pogson's scale of magnitudes.

The *second* column gives the distance in units of that of a 6th magnitude star, calculated with Pogson's ratio of $10^{0·4}$

The *third* column gives the logarithm of the number of stars, arbitrarily assuming 9·10 for the 6th magnitude to get agreement with the numbers given by Kapteyn in column 6 for stars between 5th and 6th magnitudes. See p. 54 of No. 18 of the Publications of the Astronomical Laboratory at Groningen, "On the number of stars of determined magnitude and galactic latitude," last column but one.

The *fourth* column gives the loss in visual magnitudes by scattering. The scattering in space is considered to be uniform, in which case the intensity of light I suffers diminution according to the law

$$dI = -k.I dr$$

or $$I = Ke^{-k_1 r} = K.10^{-k_2 r}.$$

$$\therefore \quad \log I = \text{const.} - k_2 r.$$

Since also $$\log I = \text{const.} - 0·4.m$$

the loss in magnitudes is of the form $k_3 r$ where k_3 is a constant which, according to Rayleigh's well-known law, varies as λ^{-4}.

For visual magnitudes it was found by trial that the value $k_3 = 0·30$ was suitable when r is expressed in units of the distance of a 6th magnitude star as in column (2).

For photographic magnitudes the value of k_3 will be greater. See remarks on the seventh column.

6. The *fifth* column gives the apparent visual magnitude on this assumption, being the simple sum of columns (1) and (4).

The *sixth* column gives Kapteyn's magnitude (visual) corresponding to the value of log N in column (3). The reference to his memoir is given above. He tabulates log N for galactic latitudes 0° to 20°, 20° to 40°, 40° to 90°, and then for 0° to 90°. To avoid raising the question as to the meaning of the Galaxy, the column for log N_{40}^{90} has been selected for use on the present occasion.

The *seventh* column shows the differences between our present assumption and Kapteyn's numbers. They are not very large between magnitudes 4·0 and 10·0, but run off at the ends. As regards the fainter stars, reasons will be given in § 19 for suspecting Kapteyn's magnitudes to be too bright. As regards the brighter stars, a reason can be assigned for the discrepancy in the existence of a solar cluster of about 60 stars of magnitudes 0 to 4; but it is too early to lay stress on this possibility.

7. The *eighth* column shows the photographic absorption in magnitudes. It was remarked above that this should be greater than the visual in the ratio of λ^{-4}. It is difficult to assign suitable exact values of λ, but easy to see what should be approximate values. If we choose the D line for visual magnitudes and G for photographic, we have almost certainly taken limits too wide. We should then have

$$\left(\frac{\lambda_D}{\lambda_G}\right)^4 = \left(\frac{59}{43}\right)^4 = (1{\cdot}38)^4 = 3{\cdot}6.$$

These limits are too wide and the number must be reduced. And for a reason given later it must be again reduced. We may consider a value 2·0 as not unreasonable.

8. Assuming, then, the factor 2, we get the eighth column; and by adding (8) and (1) we get the *ninth* column, which shows photographic magnitudes. Importance is attached to this column, since it explains a long-standing difficulty about photographic magnitudes, viz.:—How is it that by increasing exposure in a given ratio we do not get the effect equivalent to an increase of light in the same ratio? This has been a puzzle for years. It was reduced to quantitative statement by the present writer in 1905: see *Mon. Not.*, lxv. p. 755, etc. From an exhaustive study of the published Greenwich results it was found (see p. 775) that—

When the time of exposure is prolonged in the ratio of five star magnitudes the photographic gain is four magnitudes.

But on recently reviewing the life work of the late Mr. R. L. J. Ellery, it was found that he had stated this precise conclusion in 1892. (See *Mon. Not. R.A.S.*, lii. p. 265.)

The exactness of the correspondence between the results obtained in different countries, by different people using different instruments, plates, and methods, was so striking (taken also in conjunction with the fact that Dr. Schwarzschild had got very similar numbers), that attention was called to it in a brief note to the British Association at Dublin this year, and it was suggested that possibly this was a law of photographic action. Sir William

Abney had in 1893 announced the breakdown *for faint lights* of the law which would naturally be assumed (regarding exposure-time and increase of brightness as equivalent), and it seemed possible that the law might break down in a systematic manner of this kind. Against this was to be set the fact that the breakdown, as observed by Sir William Abney, was *not* systematic. (He was present at Dublin when the note above mentioned was read, and repeated his conclusion that the breakdown was not systematic.)

9. The present suggestion reconciles the facts. The definiteness of the result is due to the fact that the cause is not a photographic one, and Sir William Abney's breakdown does not apply. His breakdown may still be encountered for faint lights; but the light of the stars from (say) 5th to 10th magnitude, when concentrated by a 13-inch telescope, is *not* to be regarded as faint. It will obey the regular law announced by Sir William Abney long before he announced the breakdown in certain cases; and, accordingly, when we increase the exposure-time for such lights in the ratio of five magnitudes we *do* get five magnitudes fainter, and not four; but they are photographic magnitudes, and not visual.

For true magnitudes 5·0 and 8·0 the visual magnitudes are 5·19 and 8·75, difference 3·56; while the photographic magnitudes are 5·38 and 9·51, difference 4·13; and the ratio $4{\cdot}13/3{\cdot}56 = 1{\cdot}16$. If we take true magnitudes 6·0 and 9·0 we get, similarly, the ratio $4{\cdot}80/3{\cdot}90 = 1{\cdot}23$; and for true magnitudes 7·0 and 10·0 we get $5{\cdot}84/4{\cdot}42 = 1{\cdot}32$; mean of the three ratios 1·24.

The ratio changes throughout the table, but the discussion in *Mon. Not.*, lxv., was based on magnitudes in this part of the table; and it seems probable that the same applies to other discussions which led to the same result.

[Since this paper was written, the attention of the writer has been drawn to the valuable work of Dr. C. E. K. Mees and Mr. S. E. Sheppard, who have shown that the law of equivalence and intensity begins to break down when a certain exposure is reached, which Dr. Mees puts at about 15 minutes for such work as ours. The failure at 40 minutes, the longest exposure used in our stellar measures, is not serious. See *Investigations on the Theory of the Photographic Process* (Longmans), p. 214.]

10. It remains to justify the later portion of columns (5) and (8). Column (5) is in conflict with Kapteyn; but his fainter magnitudes depend on difficult observations, and are also subject to the criticism of § 19. It will therefore be sufficient for the present if column (8) is consistent with photographic results. Now the only way we have of testing these at present is by means of column (3). On the present hypothesis, length of exposure is a true test of photographic magnitude, and the number of stars ought then to increase with exposure, according to the law indicated by columns (3) and (8).

11. The most exhaustive counts of stars with different exposures

are those given in the Greenwich volumes, obtained from exposures of $20^{sec.}$, $3^{min.}$, $6^{min.}$, and $40^{min.}$. In the Astronomer-Royal's report for 1905 are given the figures in the first and last columns of Table II. The logarithms are taken in adjacent columns, and then the differences of these. Finally, the log (exposure) differences are divided by 0·4 to bring them to differences of photographic magnitude in the column adjacent to the differences of log N. It will be seen that the photographic magnitude increases altogether by 5·2 and log N by 1·34; ratio 3·9. Now $20^{sec.}$ exposure corresponds to about a 9th magnitude star *visual*, which is between 8·0 and 9·0 true. Between 8·0 and 10·0 true the photographic

TABLE II.

Greenwich Counts of Stars.

Exposure. Sec.	Log.	Diff.	Photog. Mag.	Diff.	Log.	No. per Square Degree ÷4·05.
20	1·30	0·96	2·4	0·68	0·51	3·2
180	2·26	0·30	0·8	0·12	1·19	15·4
360	2·56	0·82	2·0	0·54	1·31	20·3
2400	3·38				1·85	70·0

magnitude increases by 4·28 and log N by 1·20, ratio 3·6. Between 9·0 and 11·0 true, the photographic magnitude increases by 5·60, and log N by the same, 1·20; ratio 4·7. The observed ratio 3·9 lies between 3·6 and 4·7, and is thus confirmed as nearly as we may reasonably expect with our rude approximation.

12. Another test of the hypothesis here advanced is afforded by the measure of the total light of all the stars. This is represented by the expression

$$\int 4\pi \mathrm{D} r^2 dr \times \mathrm{B} r^{-2} \times 10^{-r\times 0\cdot 12} = 4\pi \mathrm{DB} \int 10^{-r\times 0\cdot 12} dr$$

$$= \text{const.} \times \left[10^{-r\times 0\cdot 12} \right],$$

the expression in brackets being taken within limits. Now, writing down the values of r for successive true magnitudes as in Table I., we have

Table III.

Total Light of the Stars.

True Mag.	r.	$10^{-r\times0\cdot12}$.	Diff.	Total No. Stars.	Diff.	Light of One Star.
0·0	0·063	·9826		1·3		
			·0099		3·9	·0025
1·0	0·10	·9727		5·2		
			·0157		15·7	·0010
2·0	0·16	·957		20·9		
			·024		62	·00038
3·0	0·25	·933		83·2		etc.
			·038		248	
4·0	0·40	·895				
			·055	331	987	
5·0	0·63	·840				
			·081	1318	3930	
6·0	1·00	·759				
			·116	5248	15640	
7·0	1·58	·643				
			·142	20890	etc.	
8·0	2·51	·501		etc.		
			·170			
9·0	3·98	·331				
			·156			
10·0	6·31	·175				
			·112			
11·0	10·00	·063				
			·051			
12·0	15·8	·012				
			·011			
13·0	25·1	·0010				
			·0004			
14·0	39·8	·0006				

13. The third column gives the value of the quantity which, taken between limits, represents the total light of the stars. If the limits are 0 and ∞ this quantity is unity. The fourth column shows the contribution of each successive true magnitude: we see that these contributions increase up to magnitude 8–9 and then fall off very rapidly. The fifth column gives the theoretical total number of stars, obtained by adding 4·62, the log of the number of square degrees on the whole sphere, to the numbers whose logarithms are given in the third column of Table I., and then finding the corresponding number. In column 6 the differences show the number belonging to each magnitude. Hence column 4 divided by column 6 shows the average light of a star between magnitude zero and 1, 1 and 2, 2 and 3, etc. These quotients, in column 7, diminish in the ratio 2·512, as they should. The light of a star of magnitude zero on this scale (on which the total light of all stars is unity) is thus about 0·004 ; in other words, the light of all the stars is about 250 times that of a star of zero magnitude. To fix the exact value we must take closer limits than whole magnitudes, for the average brightness of a whole magnitude is determined chiefly by the much more numerous faint stars. This calculation is easily made, and the number comes out about 240. Now, in vol. xiv. of the *Astrophysical Journal*, there is what is called "A rude attempt to determine the total light of all the stars," by Simon Newcomb. He found 600 times the light of a zero magnitude star, with a probable error of about one quarter of the whole amount. His estimate includes the Galaxy, which he found to be about 50 per

cent. brighter than the rest of the sky. The Galaxy has been deliberately excluded from our estimate (see § 6), and making allowance for this, his estimate is still double the number arrived at above; and though his probable error is admittedly large, the discrepancy is unsatisfactory unless it can be ascribed to some real cause.

14. One such cause we find in the variation in brilliancy of the stars. We have hitherto supposed them of uniform brilliancy, whereas we know they differ considerably. Let us imagine two superposed systems, each uniform in brilliancy and uniformly scattered, but one system being brighter than the other in the ratio of n^2 to 1. Then the stars of the brighter system which have the same *true* magnitude m will be n times as far away as those of the fainter. Let us suppose, purely by way of illustration, that the two systems contribute about equally to each *true* magnitude.

15. Now in the columns representing scattering in Table I. we are concerned with multiples of r direct. For these we are to substitute multiples of r_1 and r_2 where $r_2 = nr_1$. But the total absorption will not be altered if we keep the mean of r_1 and r_2 the same, *i.e.* if

$$r_1 + r_2 = 2r \qquad \text{and} \qquad r_2 = nr_1.$$

Thus $$r_1 = 2r/(n+1) \quad \text{and} \quad r_2 = 2nr/(n+1).$$

Let us now see how these two systems contribute to the total light of the stars. They contribute equally to the true magnitude m, that is, to the magnitudes from m to $m + dm$. The number of stars for each system is proportional to $DB^{\frac{3}{2}}$ (see § 3). Hence

$$D_1B_1^{\frac{3}{2}} = D_2B_2^{\frac{3}{2}}$$

or $$D_1 = n^3 D_2.$$

Hence the total lights, which are in the ratio D_1B_1 to D_2B_2 are as n to 1, the inverse ratio of the distances which contribute to the same magnitude.

16. This general result also enables us to compare each system with the original single system. From equation $r_1 = 2r/(n+1)$ we see that the total light of the first system is $\frac{n+1}{2}$ times that of the original system; and similarly that of the second system $\frac{n+1}{2n}$ times. If then we wish to double the total light by our subdivision we have

$$\frac{1}{2}\left(\frac{n+1}{2} + \frac{n+1}{2n}\right) = 2$$

or $$(n+1)^2 = 8n$$

or $$n = 5{\cdot}8.$$

This shows that a hypothesis of this kind will explain the discrepancy between Newcomb's observed result and that found above; but at the same time the value obtained for n is too large to be admitted without question. A value $n=3$ would have been quite natural, for we know that the stars of type II, for instance, have parallaxes more than double those of type I; and when we allow for the further scattering of each type, we might easily divide the stars into two groups, one three times as far off as the other. But the value $n=3$ only gives us an increase in total light from unity to

$$\frac{1}{2}\left(\frac{3+1}{2}+\frac{3+1}{6}\right)=1{\cdot}3\,;$$

and even $n=4$ only gives 1·6; so that, with a probable value of n, we still find Newcomb's value in excess.

17. It thus appears that the variation in brilliancy of the stars will not much affect our results, and another reason must be sought for the discrepancy. One such reason is indicated in § 21 below; and of course our numbers are subject to revision. For the present it is only suggested that the coefficients ·30 for visual magnitudes and ·60 for photographic are apparently of about the right order of magnitude. Before any attempt is made to improve them, the hypothesis itself should be tested by further observations, which can easily be made, though not without some expenditure of time. For example—

(*a*) According to the present hypothesis of scattering of light, the anomalous results of increased photographic exposure are due essentially to the different wave-lengths of the light used for photography and for visual work. Therefore they should tend to disappear when photographs are taken in yellow light such as those taken with the Yerkes 40-inch and a colour screen: for such photographs, increased exposure should be much more closely the equivalent of our visual magnitudes. Possibly material is in existence which would settle this point.

(*b*) The Greenwich counts of plates with $40^{\text{min.}}$ exposure show that log N increases more slowly for the photographs of faint stars. This is as it should be on the present hypothesis, and the increase should be slower still as we proceed to fainter stars. This can be tested by comparing different (long) exposures with powerful instruments such as the large reflectors now in use. But it seems doubtful whether existing photographs could settle the point. The accidental errors will be very large unless the photographs are of the same region on the same night, and, of course, on similar plates. Care must also be taken that, in proceeding to deal with such very faint stars, we do not get a breakdown in the law of *photographic action* such as Sir William Abney has announced.

18. Other experiments will probably suggest themselves; but the facts quoted in the present paper represent a very large amount of work, and a hypothesis which reconciles them is entitled to con-

sideration until contrary evidence is forthcoming, based on a comparable amount of work.

19. Three points are still to be noticed. The first is the reason for suspecting the later portions of Kapteyn's lists already quoted; and the reason briefly is, that they give a value for the total light of the stars sensibly in excess of Newcomb's observed value. Take, for instance, his column log N_0^{90}, giving the average number of stars per square degree over the whole sky. There are about 42,000 square degrees in the sky, and so if we add 4·62 we shall get log totals over the sky. The number of stars of any given magnitude m may be taken as approximately the difference between the values of N for $m - 0{\cdot}5$ and $m + 0{\cdot}5$; and if the light be $L(m)$ we have to sum the series

$$N(0{\cdot}5) \times L(0) + [N(1{\cdot}5) - N(0{\cdot}5)] \times L(1) + [N(2{\cdot}5) - N(1{\cdot}5)] \times L(2) + \ldots .$$
$$= N(0{\cdot}5)[L(0) - L(1)] + N(1{\cdot}5)[L(1) - L(2)] + N(2{\cdot}5)[L(2) - L(3)] + \ldots .$$

Now

$$L(0) - L(1) = L(0)[1 - {\cdot}3987] = 0{\cdot}6L(0)$$

and the succeeding factors form a geometrical progression with ratio $10^{-0{\cdot}4}$. Hence to Kapteyn's value for log N(0·5) we must add 4·62 + log 0·6 = 4·40; and to succeeding half magnitudes 4·00, 3·60, 3·20, etc., as below.

Mag.	Log N.	Add.	Sum.	No.	Mag.	Log N.	Add.	Sum.	No.
0·5	6·18	4·40	0·58	3·8	10·5	1·23	0·40	1·63	42·7
1·5	6·72	4·00	0·72	5·2	11·5	1·68	0·00	1·68	47·9
2·5	7·24	3·60	0·84	6·9	12·5	2·12	9·60	1·72	52·5
3·5	7·77	3·20	0·97	9·3	13·5	2·55	9·20	1·75	56·2
4·5	8·29	2·80	1·09	12·3	14·5	2·97	8·80	1·77	58·9
5·5	8·80	2·40	1·20	15·8	15·5	3·38	8·40	1·78	60·3
6·5	9·31	2·00	1·31	20·4	16·5	3·78	8·00	1·78	60·3
7·5	9·81	1·60	1·41	25·7	17·5	4·16	7·60	1·76	57·5
8·5	0·29	1·20	1·49	30·9	18·5	4·54	7·20	1·74	55·0
9·5	0·76	0·80	1·56	36·3					
				166·6					491·3

It will be seen that up to the 18th magnitude we have 658 times the light of a zero star, already in excess of Newcomb's measure; and the series is still far from complete.

20. Secondly, it remains to suggest a *vera causa* for the scattering of light in space. It is not necessary to go beyond experience for this: we know that tiny particles of matter are being dissipated into space from the tails of comets and from the coronas of suns like our own. If they are dispersed under the influence of light pressure, they will be repelled from all bright bodies, and thus kept hovering in space except in so far as they fall into dark

bodies. It is at any rate conceivable that a balance has been reached in course of ages between the output from bright suns and the settling down of these particles on to dark suns or planets, and that the density of the particles in space is approximately uniform and constant. Since these particles are comparable in size with a wave-length of light, they are of about the right size to give the same kind of scattering of light to which Lord Rayleigh's law of λ^{-4} applies.

21. Thirdly, it remains to state a difficulty to which my attention has been drawn by Mr. H. C. Plummer since this paper was in type, viz. that we ought to be getting scattered light from the sky, in addition to the light of the stars. If the quantity thus received can be shown to be small, there is no difficulty at all; quite the contrary, for we might thus find an explanation of the defect from Newcomb's observed value for the whole light from the sky indicated in §§ 12–17. But it should be shown why the scattered light is small. For with an infinite universe the sky would be all over as bright as the average star if no light were lost. If light is merely scattered and not lost, the sky would still be as bright, though in a different way. There is, of course, no need to postulate an infinite universe for anything that precedes; the sum to infinity in § 12 is merely a convenient abbreviation. The present hypothesis does not abolish limits to the universe, it merely removes them further away than those which would otherwise have to be assigned to fit observed facts. But the hypothesis is incomplete without some examination of the fate of the scattered light. But this cannot be undertaken at the moment without unduly delaying the press. It seems probable that the hypothesis of simple scattering alone must be modified by the inclusion of actual absorption or obstruction of light by other particles too large to scatter it. Such obstruction would, however, be the same for all wave-lengths; and as one of the main objects of the present suggestion is to explain the discordance between visual and photographic results, scattering must have a large share in the total loss. For illustration, suppose the visual loss 0·30 to be made up of 0·15 obstruction and 0·15 scattering. Then if the photographic loss remains at 0·60, it must be due to 0·15 obstruction and 0·45 scattering; so that the ratio of photographic to visual scattering would be 3 instead of 2. From § 7 we see that this is not by any means impossible. But it is not intended to lay stress on these figures.

On the Inclinations of the Planes of some Spiral Nebulæ to the Galaxy. By H. Knox Shaw, B.A.

1. The measurements of spiral nebulæ contained in this paper were made at the suggestion of Professor Turner to test whether there is any connection between the planes of these nebulæ and that of the Milky Way, as seems to be the case with binaries and long-period variables (see *M.N.*, lxvii. p. 498 *seq.*, p. 352). The general assumption is made that each nebula is circular in form and appears elliptical owing to the inclination of its plane to the line of sight. This inclination is determined by measuring the axes of the ellipse which the nebula presents to us. It is of course impossible to tell in which direction the plane is tilted, and thus two poles are found for each nebula, either of which is equally likely to be the true one. They are each at a distance γ (measured in arc) from the nebula in the direction perpendicular to the major axis of the ellipse, where $\sec \gamma = \frac{a}{b}$; a, b being the measured major and minor axes of the ellipse.

2. Measurements were made of twenty-five spiral nebulæ from Isaac Roberts' photographs. Those most open, *i.e.* most circular in form, were excluded, for two reasons: firstly, because it was impossible to orientate them with any degree of certainty; and secondly, because a small error in the ratio of the axes, when they are nearly equal, produces a large error in γ. To avoid the labour of some rather heavy computation, the positions of the two ambiguous poles were found by means of a globe and protractor. This method seemed accurate enough for the purpose, as the photographs naturally did not admit of very precise measurement, the outline of the nebula being often very ill-defined, and dependent upon personal opinion. Two independent determinations were therefore made, at an interval of about a fortnight, in order that the second should not be in any way influenced by the first. It will be seen that in the case of some nebulæ the opinion as to their form changed considerably during the interval. The two determinations are throughout the paper shown bracketed, and in discussing the results the means of the two determinations are used.

3. In Table I. the measurements of the photographs will be found. The first column gives the name of the nebula, the second and third its R.A. and declination, the fourth its galactic latitude, the fifth and sixth the axes of the ellipse, the seventh the arc γ, and the eighth the angle θ, this being the inclination of the major axis to the hour circle, considered positive when the south end is turned in the direction of increasing R.A.

TABLE I.

Measurements of the Nebulæ.

Nebula.	R.A. h	R.A. m	Dec. °	Gal. Lat. °	*a.* in.	*b.* in.	γ. °	θ. °
M 31	0	37	+41	−22	6·20	1·50	76	−35
					6·30	1·40	77	−34
M 33	1	28	+30	−31	1·50	1·30	30	−43
					3·10	1·50	61	−14
H V. 19	2	16	+42	−17	0·97	0·11	83	−20
					0·98	0·13	82	−20
H V. 44	7	27	+66	+30	0·90	0·55	52	+58
					1·10	0·80	44	+53
H I. 56	9	27	+22	+46	0·60	0·35	54	−12
					0·60	0·28	62	−11
H I. 285	9	39	+68	+41	0·61	0·21	70	+38
					0·66	0·25	68	+38
M 81	9	47	+70	+41	1·30	0·70	58	+36
					1·30	0·75	55	+35
H V. 47	9	55	+56	+48	0·90	0·15	80	+19
					0·90	0·12	82	+21
H I. 199	10	14	+46	+55	1·10	0·32	72	−40
					1·20	0·34	74	−38
H V. 46	11	6	+56	+56	1·10	0·25	77	−81
					1·10	0·20	80	−83
M 65	11	14	+14	+66	1·16	0·30	75	+ 7
					1·25	0·40	74	+ 7
M 66	11	15	+14	+66	1·05	0·40	68	+ 2
					1·20	0·65	71	+ 2
H II. 730	11	28	+48	+65	0·91	0·63	46	−12
					0·94	0·65	46	−14
H IV. 56	11	58	+45	+70	0·60	0·25	65	+46
					0·60	0·35	54	+60
H I. 206	12	0	+51	+65	0·88	0·28	71	−45
					0·91	0·20	77	−45
H V. 41	12	12	+38	+78	2·30	0·20	85	−46
					2·30	0·20	85	−45
M 99	12	14	+15	+76	0·65	0·52	37	−31
					0·60	0·50	34	−36
H V. 2	12	29	+ 3	+64	1·35	0·40	73	+55
					1·15	0·45	67	+55

TABLE I.—*continued.*

Nebula.	R.A. h	m	Dec. °	Gal. Lat. °	*a*. in.	*b*. in.	γ. °	θ. °
H I. 92	12	31	+28	+87	0·70	0·26	69	+40
					0·70	0·28	66	+40
H V. 24	12	31	+27	+87	2·80	0·22	86	+47
					2·70	0·20	86	+48
H V. 42	12	37	+33	+84	2·10	0·25	83	−84
					2·15	0·22	84	−80
H I. 84	12	46	+26	+88	0·90	0·50	56	−42
					1·15	0·80	46	−42
M 64	12	52	+22	+84	1·30	0·60	63	+75
					1·20	0·55	63	+71
M 63	13	11	+43	+73	1·20	0·60	60	+86
					1·20	0·65	57	+86
H IV. 76	20	33	+60	+11	0·70	0·50	44	−24
					1·00	0·60	53	−39

4. The second and third columns of Table II. give the R.A. and declination of one pole of the nebula, and the fourth column the corresponding galactic latitude (Γ), the fifth, sixth, and seventh giving the similar quantities for the alternative pole. Γ′ is in each case the smaller of the galactic latitudes. The galactic longitudes of the poles were also calculated, to see whether there was any sign of clustering in longitude. As no such clustering was apparent, it seemed unnecessary to include them in the table.

TABLE II.

Positions of the Poles of the Nebulæ.

Nebula.	R.A. h	m	Dec. °	Γ. °	R.A. h	m	Dec. °	Γ′. °
M 31	17	56	+34	+24	3	56	−22	−46
	17	12	+41	+34	4	20	−16	−39
M 33	23	16	+48	−12	2	56	+ 8	−42
	20	20	+33	− 3	5	24	+ 3	−16
H V. 19	7	20	− 7	+ 5	19	4	+27	+ 7
	6	56	−10	− 1	19	16	+28	+ 3
H V. 44	6	4	+17	0	17	28	+63	+33
	5	54	+25	0	15	36	+63	+44
H I. 56	5	28	+26	− 3	13	0	+ 4	+66
	5	0	+25	− 9	13	36	+ 2	+60
H I. 285	6	28	+ 6	0	17	44	+33	+25
	6	12	+ 9	− 2	17	40	+34	+27

TABLE II.—*continued.*

Nebula.	R.A.		Dec.	I.	R.A.		Dec.	I'.
	h	m	°	°	h	m	°	°
M 81	6	36	+19	+ 8	17	36	+43	+30
	6	40	+23	+10	17	32	+45	+31
H V. 47	4	52	+ 2	−23	17	8	+24	+30
	5	56	− 1	−11	18	0	+26	+20
H I. 199	2	44	+44	−13	13	36	−12	+48
	2	44	+42	−15	13	48	−11	+48
H V. 46	23	40	+46	−15	11	44	−20	+41
	23	8	+44	−15	11	28	−24	+35
M 65	6	30	− 5	− 5	16	18	+ 6	+34
	6	20	+ 2	− 4	16	20	+14	+38
M 66	6	36	+ 4	+ 1	15	52	+ 7	+41
	6	24	+ 7	− 1	16	4	+ 8	+38
H II. 730	7	4	+38	+21	14	52	+24	+61
	6	52	+43	+20	15	0	+25	+59
H IV. 56	19	4	+49	+16	9	20	−11	+28
	19	0	+67	+23	10	28	− 5	+44
H I. 206	3	52	+47	− 4	15	4	− 8	+40
	3	24	+42	−11	15	12	−15	+34
H V. 41	3	8	+37	−12	15	12	−19	+31
	3	40	+43	− 8	16	0	−28	+17
M 99	14	22	− 5	+49	9	48	+32	+52
	14	8	− 5	+51	10	4	+33	+56
H V. 2	8	32	−50	− 5	16	52	+54	+37
	8	44	−42	+ 2	16	8	+52	+45
H I. 92	18	32	+49	+22	9	56	16	+31
	18	12	+57	+26	9	8	15	+23
H V. 24	20	20	+37	− 1	8	8	−34	+ 1
	20	24	+45	+ 3	8	20	−37	+ 1
H V. 42	0	36	+63	0	13	32	−50	+12
	0	52	+62	− 1	13	28	−51	+11
H I. 84	15	24	−13	+33	8	12	+52	+35
	15	4	− 7	+41	9	8	+48	+44
M 64	11	0	−39	+20	16	16	+78	+34
	11	28	−39	+21	17	20	+74	+32
M 63	1	20	+77	+14	12	48	−17	+45
	1	12	+80	+17	13	0	−15	+47
H IV. 76	23	36	+27	−33	15	16	+55	+51
	22	36	+14	−38	12	28	+54	+62

5. A rough inspection of Table II. will show that, with few exceptions, one of the ambiguous poles is quite near the galaxy. It will be instructive to compare the distribution of these poles with what might be called a chance distribution. Now, in the first place, let the general assumption be made that the nebulæ are distributed uniformly in galactic latitude, and that their planes are inclined at every sort of angle to the line of sight, *i.e.* that the values of γ are distributed uniformly between 0° and 90°, and values of θ uniformly between $-90°$ and $+90°$. Then the chance that any single pole shall lie between galactic latitudes 0 and α is $\sin\alpha$. Hence the chance that at least one of a pair of poles shall be within a distance α of the galaxy is $1-(1-\sin\alpha)^2$ or $2\sin\alpha-\sin^2\alpha$. From this are derived the numbers in the third column of Table III., while the second column gives the corresponding numbers derived from the figures in the column of Table II. headed Γ; the fourth column shows the excess of the observed over the calculated. It will be noticed that there is a decided excess of poles within ten degrees of the galaxy.

Table III.

Distribution of Poles.

Gal. Lat.	O.	C.	O-C.
0°–10°	13	8	+5
10–20	5	6	−1
20–30	4	5	−1
30–40	2	3	−1
40–50	1	2	−1
50–90	0	1	−1

6. We have seen that, on the assumption of a uniform distribution of the nebulæ with regard to the galactic plane and also of their orientations, there are more poles near the galaxy than can be accounted for by the theory of probability. We must now consider how far these assumptions of uniformity are true with regard to the twenty-five nebulæ under discussion. The distribution of the nebulæ with regard to the galactic plane is best seen by reference to Table IV., where the second column gives the number of nebulæ in the various galactic zones, the third the number there would be in each zone were the distribution uniform, and the fourth the excess of the former over the latter. It will at once be seen that the nebulæ are by no means distributed uniformly, there being no less than fourteen within 30° of the galactic pole.

TABLE IV.

Distribution of Nebulæ.

Gal. Lat.	O.	C.	O-C.
0°-40°	5	16	−11
40–50	4	3	+ 1
50–60	2	3	− 1
60–70	5	2	+ 3
70–80	4	1	+ 3
80–90	5	0	+ 5

7. Table V. has been formed to show in a similar way the distribution of the values of γ. Inspection of the table will show that half of the nebulæ have a value of γ greater than 70°.

TABLE V.

Distribution of Values of γ.

Values of γ.	Number.
0°–30°	0
30–40	1
40–50	4
50–60	4
60–70	4
70–80	7
80–90	5

8. Thus, so far from the nebulæ being placed and orientated uniformly, they show a decided tendency to cluster near the galactic pole, fourteen of them being within 30 degrees of it; and the values of γ are large, twelve being greater than 70 degrees. The absence of small values of γ is due to the fact that the more open nebulæ were not measured, for the reasons given above. The combination of these two properties (as shown by Tables IV. and V.) accounts for a great number of the small galactic latitudes of the poles. Thus it is difficult to decide whether the clustering of the poles near the galaxy may not be due to the fact that the majority of the nebulæ lie near the galactic pole, and are tilted at a considerable angle to the line of sight. The fact that the galactic latitudes of five nebulæ which were considered to be too open to admit of measurement—and would thus have small values of γ—range from 45 to 78 degrees, shows that, could these nebulæ have been included in the determination, their poles would have been found to lie in large galactic latitudes. It seems clear that more evidence is required before any definite opinion can be advanced as to whether there is any real clustering of the poles near the galaxy. At present I do not think that anything more can be said than that the result of the investigation has been positive rather than negative, and consequently that it will be well worth continuing it by measuring all available photographs of spiral nebulæ.

The Short Period Variable W Ursæ Majoris.

By J. M. Baldwin, M.A., 1851 Exhibition Scholar (Melbourne).

(*Communicated by Sir David Gill, K.C.B., F.R.S.*)

Observations of W Ursæ Majoris (B.D. + 56°, 1400, $\alpha = 9^h\ 36^m\ 44^s$, $\delta = 56°\ 24'{\cdot}6$, 1900), with the photometer D attached to the Steinheil refractor of the Potsdam Astrophysical Observatory, have been made by me during the last year as often as opportunity offered. This interesting variable, with a period of only four hours, was discovered by Müller and Kempf at Potsdam, and these observers have already published a light curve and ephemeris,* but it seemed desirable to have additional data for the light curve, and also of interest to observe some additional minima. In all, 263 observations were made, in each case the comparison star used being B.D. + 54°, 1329 ($\alpha = 9^h\ 41^m\ 44^s$, $\delta = 54°\ 43'{\cdot}7$, 1900), the magnitude of which from the Potsdam *Durchmusterung* observations is 7·73. As usual, each observation consisted of four settings for the variable star and four for the comparison star in alternate observations, the variable being observed before and after the comparison star.

In Table I. the complete observations are given, the columns containing successively the date and Potsdam sidereal time of observation, Greenwich mean time (with reduction to Sun applied), and the observed magnitude (corrected for atmospheric absorption). The remaining columns contain elements computed as explained later.

TABLE I.

Date. 1907.	Sid. Time. h m	G.M.T. h m	Mag.	C.	O—C.	Prec. Min. h m	Epoch.
May 29	13 54	8 33	8·08	8·16	− ·08	7 53	9568
	14 11	50	7·86	8·05	− ·19		
	18	57	7·95	8·03	− ·08		
	33	9 12	8·09	7·99	+ ·10		
	15 10	49	8·02	7·91	+ ·11		
	22	10 1	7·85	7·90	− ·05		
	27	6	7·86	7·90	− ·04		
	43	22	7·81	7·91	− ·10		
	53	32	7·77	7·93	− ·16		
June 5	14 22	8 33	8·51	8·27	+ ·24	8 2	9610
	37	48	7·92	8·11	− ·19		
	47	58	7·97	8·05	− ·08		
	15 7	9 18	8·11	8·00	+ ·11		
	16	27	7·98	7·98	·00		
	30	41	7·88	7·95	− ·07		

* *Ap. J.*, vol. 17, No. 201 (1903); *A.N.* 4005, 167, 347 (1905), and *A.N.* 4128, 172, 387 (1906).

TABLE I.—*continued.*

Date. 1907.	Sid. Time. h m	G.M.T. h m	Mag.	C.	O—C.	Prec. Min. h m	Epoch.
June 8	14 40	8 39	8·19	8·24	−·05	8 6	9628
	52	51	8·21	8·11	+·10		
	15 4	9 3	8·05	8·05	·00		
	17	16	7·87	8·01	−·14		
June 9	14 15	8 10	8·57	8·49	+·08	8 8	9634
	27	22	8·40	8·43	−·03		
	38	33	8·25	8·32	−·07		
	53	48	8·32	8·16	+·16		
	15 2	57	8·22	8·08	+·14		
	34	9 29	8·01	7·99	+·02		
June 19	14 54	8 9	8·59	8·41	+·18	4 21	9693
	15 13	28	8·57	8·47	+·10	8 21	9694
	19	34	8·55	8·44	+·11		
	31	46	8·41	8·32	+·09		
	38	53	8·20	8·25	−·05		
	51	9 6	7·96	8·11	−·15		
	57	12	7·96	8·07	−·11		
	16 23	38	8·03	7·99	+·04		
July 18	16 51	8 10	8·26	8·06	+·20	4 59	9867
	17 4	23	8·07	8·16	−·09		
	12	31	8·18	8·23	−·05		
	22	41	8·32	8·34	−·02		
	27	46	8·42	8·40	+·02		
	37	56	8·66	8·48	+·18		
	41	9 0	8·51	8·49	+·02	8 59	9868
	53	12	8·59	8·44	+·15		
	18 0	19	8·45	8·38	+·07		
	17	36	8·10	8·20	−·10		
July 22	17 20	8 24	8·23	8·12	+·11	5 4	9891
	30	34	8·18	8·20	−·02		
	33	37	8·17	8·23	−·06		
	44	48	8·38	8·35	+·03		
	48	52	8·47	8·40	+·07		
	18 4	9 7	8·51	8·49	+·02	9 5	9892
	11	14	8·27	8·46	−·19		
	21	24	8·29	8·38	−·09		
	26	29	8·24	8·32	−·08		
	35	38	8·22	8·23	−·01		
	44	47	8·06	8·13	−·07		
	56	59	8·07	8·06	+·01		

TABLE I.—*continued.*

Date. 1907.	Sid. Time. h m	G.M.T. h m	Mag.	C.	O—C.	Prec. Min. h m	Epoch.
July 25	17 9	8 1	7·97	7·97	·00	5 8	9909
	19	11	8·22	8·01	+·21		
	23	15	8·04	8·03	+·01		
	32	24	8·23	8·09	+·14		
	39	31	8·04	8·14	−·10		
	50	42	8·35	8·23	+·12		
	57	49	8·37	8·31	+·06		
	18 8	9 0	8·34	8·44	−·10		
	12	4	8·24	8·47	−·23		
Aug. 6	18 40	8 44	8·18	8·12	+·06	5 24	9981
	48	52	8·03	8·19	−·16		
	51	55	8·28	8·22	+·06		
	19 0	9 4	8·22	8·31	−·09		
	6	10	8·42	8·39	−·03		
	15	19	8·45	8·47	−·02		
	22	26	8 24	8·49	−·25	9 25	9982
	31	35	8·32	8·45	−·13		
	34	38	8·25	8·43	−·18		
	46	50	8·08	8·31	−·23		
	53	57	7·98	8·24	−·26		
	20 5	10 9	8·08	8·11	−·03		
Aug. 11	17 44	7 29	7·85	7·91	−·06	5 31	10011
	53	38	7·88	7·90	−·02		
	59	44	7·84	7·90	−·06		
	18 10	55	7·88	7·90	−·02		
	17	8 2	7·85	7·91	−·06		
	26	11	7·99	7·93	+·06		
	19 1	46	7·92	8·08	−·16		
	11	56	8·08	8·16	−·08		
Aug. 28	19 2	7 41	7·91	7·93	−·02	5 54	10113
	8	47	8·02	7·92	+·10		
	22	8 1	7·86	7·90	−·04		
Oct. 8	1 3	11 3	8·41	8·43	−·02	10 48	10360
	13	13	8·49	8·32	+·17		
	17	17	8·43	8·28	+·15		
	29	29	8·26	8·15	+·11		

TABLE I.—*continued.*

Date. 1907.	Sid. Time. h m	G.M.T. h m	Mag.	C.	O—C.	Prec. Min. h m	Epoch.
Dec. 6	4 24	10 37	7·94	7·91	+·03	8 6	10713
	36	49	7·94	7·93	+·01		
	42	55	7·82	7·95	−·13		
	53	11 6	8·06	8·00	+·06		
1908.							
Jan. 1	4 55	9 28	8·21	8·09	+·12	8 41	10869
	5 9	42	8·10	8·03	+·07		
	18	51	8·03	8·01	+·02		
	39	10 12	8·08	7·97	+·11		
Jan. 2	4 38	9 7	8·42	8·32	+·10	8 42	10875
	51	20	8·26	8·18	+·08		
	58	27	8·15	8·11	+·04		
	5 11	40	8·03	8·04	−·01		
	18	47	8·11	8·02	+·09		
	33	10 2	7·99	7·99	·00		
	44	13	8·04	7·97	+·07		
Jan. 14	3 41	7 23	7·88	7·91	−·03	4 58	10946
	52	34	7·98	7·92	+·06		
	55	37	7·96	7·93	+·03		
	4 8	50	7·89	7·97	−·08		
	14	56	7·99	7·99	·00		
	25	8 7	7·94	8·05	−·11		
	32	14	8·14	8·10	+·04		
	43	25	8·22	8·19	+·03		
	52	34	8·27	8·27	·00		
	5 6	48	8·60	8·43	+·17		
	11	53	8·33	8·47	−·14		
	27	9 9	(8·73)	8·45	(+·28)	8 58	10947
	36	18	(8·62)	8·37	(+·25)		
Jan. 25	3 46	6 45	8·03	7·96	+·07	5 12	11012
	51	50	7·85	7·95	−·10		
	4 1	7 0	7·91	7·93	−·02		

TABLE I.—*continued.*

Date. 1908.	Sid. Time. h m	G.M.T. h m	Mag.	C.	O−C.	Prec. Min. h m	Epoch.
Mar. 22	12 15	11 26	8·14	8·05	+ ·09	10 28	11355
	26	37	8·11	8·01	+ ·10		
	31	42	7·92	8·00	− ·08		
	42	53	7·95	7·98	− ·03		
	48	59	7·93	7·97	− ·04		
	58	12 9	7·89	7·94	− ·05		
	13 4	15	7·88	7·93	− ·05		
	16	27	7·91	7·91	·00		
Mar. 24	12 9	11 12	8·13	8·15	− ·02	10 31	11367
	21	24	8·10	8·06	+ ·04		
	26	29	8·10	8·04	+ ·06		
	41	44	7·82	8·00	− ·18		
	49	52	7·95	7·99	− ·04		
	13 4	12 7	7·91	7·95	− ·04		
	10	13	7·86	7·94	− ·08		
	25	28	7·93	7·91	+ ·02		
Mar. 25	12 6	11 5	8·27	8·24	+ ·03	10 32	11373
	18	17	8·07	8·11	− ·04		
	25	24	8·00	8·07	− ·07		
	36	35	7·86	8·03	− ·17		
	41	40	7·97	8·02	− ·05		
	58	57	7·98	7·98	·00		
Apr. 15	12 46	10 20	8·36	8·13	+ ·23	7 0	11498
	54	28	8·19	8·19	·00		
	59	33	8·48	8·24	+ ·24		
	13 8	42	8·46	8·35	+ ·11		
	18	52	8·55	8·45	+ ·10		
	29	11 3	8·55	8·49	+ ·06	11 0	11499
	34	8	8·57	8 47	+ ·10		
	44	18	8·62	8·39	+ ·23		
	50	24	8·29	8·33	− ·04		
	59	33	8·25	8·23	+ ·02		
	14 3	37	8·31	8·19	+ ·12		
	13	47	8·09	8·09	·00		
	21	55	8·04	8·05	− ·01		
	30	12 4	8·01	8·03	− ·02		

TABLE I.—*continued.*

Date. 1908.	Sid. Time. h m	G.M.T. h m	Mag.	C.	O–C.	Prec. Min. h m	Epoch.
Apr. 26	13 23	10 13	7·83	7·99	·16	7 14	11564
	31	21	7·96	8·03	– ·07		
	37	27	7·98	8·07	– ·09		
	46	36	8·00	8·13	·13		
	51	41	8·13	8·17	– ·04		
	14 1	50	8·13	8·26	– ·13		
	6	55	8·32	8·32	·00		
	15	11 4	8·31	8·43	– ·12		
	21	10	8·42	8·47	– ·05		
	30	19	8·44	8·48	– ·04	11 15	11565
	35	24	8·43	8·46	– ·03		
	45	34	8·29	8·38	– ·09		
	57	46	8·24	8·25	– ·01		
	15 6	55	8·06	8·15	– ·09		
	12	12 1	8·15	8·10	+ ·05		
	21	10	8·07	8·05	+ ·02		
	30	19	8·06	8·03	+ ·03		
	40	29	7·98	8·00	– ·02		
May 3	14 8	10 29	8·15	8·03	+ ·12	7 24	11606
	17	38	7·95	8·08	– ·13		
	22	43	8·06	8·12	– ·06		
	31	52	8·15	8·19	– ·04		
	38	59	8·12	8·26	– ·14		
	47	11 8	8·35	8·37	– ·02		
	54	15	8·45	8·44	+ ·01		
	15 3	24	8·34	8·49	– ·15	11 24	11607
	11	32	8·53	8·47	+ ·06		
	20	41	8·37	8·40	– ·03		
	28	49	8·29	8·32	·03		
	38	59	8·26	8·21	+ ·05		
	48	12 9	7·95	8·11	– ·16		
	58	19	8·07	8·05	+ ·02		

TABLE I.—*continued.*

Date. 1908.	Sid. Time. h m	G.M.T. h m	Mag.	C.	O—C.	Prec. Min. h m	Epoch.
May 14	14 48	10 24	7·99	7·94	+ ·05	7 38	11672
	57	33	8·01	7·97	+ ·04		
	15 0	36	7·96	7·99	− ·03		
	8	44	8·03	8·03	·00		
	15	51	8·04	8·07	− ·03		
	22	58	8·13	8·12	+ ·01		
	31	11 7	8·19	8·20	− ·01		
	40	16	8·18	8·29	− ·11		
	45	21	8·39	8·35	+ ·04		
	52	28	8·37	8·43	− ·06		
	55	31	8·44	8·45	− ·01		
	16 4	40	8·47	8·49	− ·02	11 39	11673
	11	47	8·44	8·46	− ·02		
	21	57	8·47	8·39	+ ·08		
	30	12 6	8·21	8·29	− ·08		
	38	14	8·27	8·21	+ ·06		
	48	24	8·18	8·11	+ ·07		
	57	33	8·09	8·06	+ ·03		
	17 2	38	8·03	8·04	− ·01		
	13	49	8·06	8·01	+ ·05		
May 20	15 14	10 26	7·89	7·93	− ·04	7 46	11708
	21	33	8·05	7·95	+ ·10		
	24	36	7·95	7·96	− ·01		
	33	45	7·90	7·99	− ·09		
	38	50	7·95	8·02	− ·07		
	48	11 0	8·07	8·08	− ·01		
	52	4	8·11	8·11	·00		
May 20	17 53	13 5	8·08	7·99	+ ·09	11 46	11709
	55	7	8·04	7·99	+ ·05		
	18 4	16	7·98	7·97	+ ·01		
	7	19	7·98	7·96	+ ·02		
	15	27	8·02	7·95	+ ·07		
	18	30	7·87	7·94	− ·07		
	28	40	7·96	7·92	+ ·04		
	31	43	7·91	7·91	·00		
	39	51	7·88	7·90	− ·02		
	42	54	7·95	7·90	+ ·05		

TABLE I.—*continued.*

Date. 1908.	Sid. Time. h m	G.M.T. h m	Mag.	C.	O—C.	Prec. Min. h m	Epoch.
May 30	14 40	9 12	8·04	8·01	+ ·03	8 0	11768
	46	18	7·87	7·99	− ·12		
	48	20	8·00	7·99	+ ·01		
	54	26	8·02	7·97	+ ·05		
	56	28	7·88	7·97	− ·09		
	15 2	34	8·02	7·96	+ ·06		
	8	40	7·93	7·94	− ·01		
	15	47	7·89	7·93	− ·04		
	18	50	7·98	7·92	+ ·06		
	25	57	7·88	7·91	− ·03		
	30	10 2	7·96	7·90	+ ·06		
	37	9	7·86	7·90	− ·04		
	42	14	7·93	7·90	+ ·03		
	49	21	7·90	7·90	·00		
	54	26	7·93	7·91	+ ·02		
	16 1	33	8·09	7·91	+ ·18		
	2	34	7·91	7·92	− ·01		
	8	40	8·06	7·93	− ·13		
May 31	15 2	9 30	7·96	7·97	− ·01	8 1	11774
	6	34	7·97	7·96	+ ·01		
	8	36	7·90	7·96	− ·06		
	14	42	7·96	7·94	+ ·02		
	17	45	7·94	7·94	·00		
	24	52	(8·21)	7·92	(+ ·29)		
	27	55	7·95	7·92	+ ·03		
	32	10 0	7·92	7·91	+ ·01		
	40	8	7·95	7·90	+ ·05		
	46	14	7·88	7·90	− ·02		
June 1	15 34	9 58	7·97	7·91	+ ·06	8 2	11780
	35	59	7·93	7·91	+ ·02		
	41	10 5	7·95	7·90	+ ·05		
	45	9	7·94	7·90	+ ·04		
	49	13	7·90	7·90	·00		
	51	15	7·85	7·90	− ·05		
	16 11	35	7·94	7·91	+ ·03		
	18	42	8·00	7·93	+ ·07		

July 18. Clouded over suddenly at $18^h\ 30^m$.
July 25. Very hazy near horizon.
Aug. 11. Hazy near horizon. Mar. 22. Not very clear.
Apr. 15. Sky white throughout. Better during the second half.
May 3. Unsteady throughout, becoming hazy for last two observations.
May 20. (Second series.) Field somewhat bright. Rather unsteady.
May 30. At first somewhat unsteady, but improving later.
May 31. Good till $15^h\ 50^m$, then hazy.

The approximate form of the light curve near minima was obtained from the observations of 1908 April 26 and May 14, using the period as determined by Müller and Kempf ($4^h\ 0^m\ 13^s{\cdot}21$), and by means of this curve the minima given in Table II. were deduced. For convenience, minima previously observed by Müller and Kempf are given also, together with C_1 the times of minima computed from their formula, and the difference O—C_1.

TABLE II.

Minima.

Date.	G.M.T.	Epoch.	Weight.	C_1.	O—C_1.	C_2.	O—C_2.
	h m			h m	m	h m	m
1903 Jan. 14	4 38	0	3	4 39	− 1	4 37	+1
Feb. 20	5 31	222	1	5 28	+ 3	5 27	+4
Apr. 24	10 51	601	2	10 51	0	10 50	+1
1904 Feb. 13	5 20	2368	1	5 20	0	5 21	−1
1905 Jan. 17	8 49	4401	2	8 48	+ 1	8 51	−2
1906 July 26	9 1	7728	2	9 1	0	9 7	−6
1907 July 18	9 1	9868	1	8 52	+ 9	9 0	+1
22	9 1	9892	1	8 57	+ 4	9 3	−2
Aug. 6	9 17	9982	1	9 17	0	9 25	−8
1908 Apr. 26	11 18	11565	2	11 5	+13	11 14	+4
May 3	11 27	11607	1	11 14	+13	11 23	+4
14	11 42	11673	2	11 29	+13	11 38	+4

The minimum 11499 on 1908 April 15 has not been used for computing the period, on account of the unfavourable conditions during the first half. It was at first intended to reject these observations, but finally it was decided to let them remain.

From these minima, the correction to the epoch and period was determined by the method of least squares, the result being

$$\text{Heliocentric Minimum} = 1903\ \text{Jan.}\ 14^d\ 4^h\ 37^m{\cdot}4\ \text{G.M.T.} + 4^h\ 0^m\ 13^s{\cdot}267\ E.$$

Using these elements, the epoch and phase of each observation was computed, and then the observations were arranged in order of phase. Next, they were grouped to give 24 normal points (see Table III.), each point representing 10, 11, or 12 observations. These normal points were plotted and the light curve drawn. This curve was then used to obtain new values for the times of minimum brightness, but the alterations were so slight (+ 2 min. for 1907 July 18 and + 1 min. for 1908 April 26) that the elements could be taken as definitive.

TABLE III.

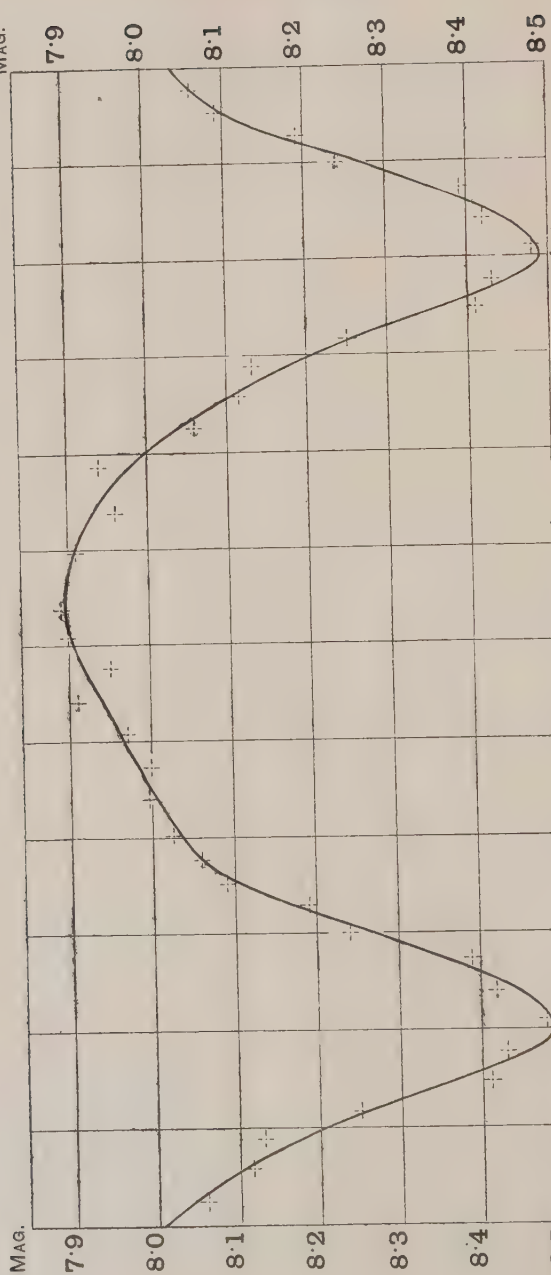
Normal Points for Curve.

Phase. h	m	Mag.	Curve.	O—C.	Phase. h	m	Mag.	Curve.	O—C.
0	3·2	8·48	8·49	− ·01	1	53·4	7·95	7·92	+ ·03
	11·5	8·42	8·45	− ·03	2	2·3	7·90	7·90	·00
	21·4	8·39	8·36	+ ·03		10·9	7·89	7·90	− ·01
	29·8	8·24	8·27	− ·03		28·9	7·91	7·91	·00
	38·4	8·19	8·17	+ ·02		41·2	7·96	7·93	+ ·03
	45·5	8·09	8·11	− ·02		55·4	7·94	7·98	− ·04
	53·5	8·06	8·06	·00	3	7·6	8·06	8·04	+ ·02
1	0·7	8·02	8·03	− ·01		17·7	8·11	8·11	·00
	12·1	7·99	8·00	− ·01		26·7	8·13	8·17	− ·04
	21·6	8·00	7·98	+ ·02		35·2	8·25	8·25	·00
	32·0	7·97	7·96	+ ·01		44·4	8·41	8·37	+ ·04
	42·0	7·91	7·94	− ·03		53·1	8·43	8·45	− ·02

The times for minima were computed from the above formula and are given in column C_2 of Table II., together with the difference between the observed and computed times $O—C_2$.

Looking at the column $O—C_1$ in Table II., one is at once struck by the three values +13 min. from the 1908 observations. Of these, the minima of April 26 and May 14 were particularly good series of observations and agree very well together, not only in the time of minimum, but also in the form of the curve that they give, and it is unlikely that the time of minimum derived from them can be in error by more than 1 or 2 minutes. In column $O—C_2$ these observations again give a somewhat large positive difference of +4 min., and the value −6 min. from the minimum of 1906 July 26 also calls for special attention. Professor Müller kindly placed the original observations at my disposal, but no correction to the time of minimum such as to make the difference smaller appeared to be feasible; in fact, I was inclined to place the time of minimum 1 or perhaps 2 minutes earlier. This difference, then, as well as that for the 1908 observation, appears greater than can be accounted for by errors of observation. The difference for 1907 August 6 is also large.

This naturally suggests the question as to whether the period is constant or not, the period apparently having increased during the latter part of the series. The data, however, were not considered sufficient to enter into this question. Another point that may be noticed is that the two large negative differences occur at even epochs, while the large positive differences occur at odd ones. This would suggest that the intervals between successive minima were different, and consequently the observations were divided up into two sets, namely, those between odd and even minima, and those between even and odd; from each set 20 normals were


Mag.
7·9
8·0
8·1
8·2
8·3
8·4
8·5
0
1
2
3
4 hours.
W Ursæ Majoris.

formed and the light curves plotted, but no difference could be determined between the two curves, so that the observations give no evidence of such irregularity.

The light curve (see figure, and Table IV.) shows that the variation in light is continuous, and that the curve for three-quarters of an hour on each side of the minimum is nearly symmetrical; then, however, the ascending branch turns somewhat sharply, and the increase to maximum is much slower than the decrease after maximum. The maximum occurs about 2^h 10^m after minimum, and is much less sharply defined. If the curve be compared with that given by Müller and Kempf it will be seen—

(1) The amplitude of the present curve is less.

(2) The minimum is less sharply curved.

(3) The gradual rise to maximum agrees with their suspected depression in the curve.

(4) The curves after maximum till near minimum are practically identical.

TABLE IV.

Magnitudes read from Curve.

Time after Minimum.	Mag.	Time after Minimum.	Mag.	Time after Minimum.	Mag.	Time after Minimum.	Mag.
h m		h m		h m		h m	
0 0	8·49	1 0	8·04	2 0	7·91	3 0	8·00
5	8·48	5	8·02	5	7·90	5	8·02
10	8·46	10	8·01	10	7·90	10	8·05
15	8·42	15	8·00	15	7·90	15	8·09
20	8·37	20	7·99	20	7·90	20	8·12
25	8·32	25	7·98	25	7·90	25	8·16
30	8·27	30	7·97	30	7·91	30	8·21
35	8·21	35	7·96	35	7·92	35	8·25
40	8·16	40	7·95	40	7·93	40	8·31
45	8·11	45	7·94	45	7·94	45	8·37
50	8·08	50	7·92	50	7·96	50	8·43
55	8·05	55	7·91	55	7·97	55	8·47
1 0	8·04	2 0	7·91	3 0	8·00	4 0	8·49

From the light curve, the magnitude corresponding to each observation was read off. These are the computed magnitudes given in Table I., in which the difference observed magnitude—computed magnitude is also given. In three cases where the difference is large, the numbers have been enclosed in brackets. These observations have not been used at all. Two of these observations were made at the end of an evening's observing in an inconvenient position, which may account for the large errors; the third is probably due to some change in the clearness of the sky.

For the sake of completeness, the whole of the minima of W

Ursæ Majoris which have been already published by other observers is given in the following Table V., together with the times C_1 computed from the formula given by Müller and Kempf, and C_2 those computed from the formula obtained in this paper.

TABLE V.

Additional Minima.

Observer.	Epoch.	Date.	G.M.T.	C_1.	O—C_1.	C_2.	O—C_2.	Observers' Remarks.
			h m	h m	m	h m	m	
Fauth *	2165	1904 Jan. 10	8 56	8 36	+20	8 36	+20	
	2207	17	9 5	8 45	+20	8 45	+20	
	2266	27	5 23	4 58	+25	4 58	+20	Uncertain.
	2267		9 18	8 58	+20	8 59	+19	Good.
	2393	Feb. 17	9 46	9 26	+20	9 26	+20	Somewhat uncertain.
	2404	19	5 49	5 28	+21	5 29	+20	Somewhat uncertain.
	2548	Mar. 14	6 34	6 0	+34	6 1	+33	Somewhat uncertain.
Parkhurst	6178	1905 Nov. 9	19 16	19 19	− 3	19 23	− 7	Fair plate.
and Jordan†	6346	Dec. 7	20 11	19 56	+15	20 0	+11	Two minima, plates very poor and poor.
Tikhoff ‡	9052	1907 Mar. 4			+ 5		− 2	Orange filter.
	to	to						
	9407	May 2			+15		+ 8	Ultra violet filter.

* Ph. Fauth, *A.N.* 3963, 1904.
† J. A. Parkhurst and F. C. Jordan, *Ap. J.*, vol. 23, p. 82, 1906.
‡ G. A. Tikhoff, *Pulk. Mitteil.*, Bd. ii. No. 21, 1908.

Fauth's observations were estimations, Parkhurst and Jordan obtained their minima from photographs, and Tikhoff's also were from photographs, colour filters being used. The large positive residuals given by Fauth's observations are remarkable, but the minimum observed by Müller and Kempf during this period with the Zöllner photometer gives residual zero. The times given by Tikhoff indicate that the minima fall later than given by Müller and Kempf's ephemeris, a result agreeing with the observations given in this paper.

In addition, 13 photometric measures during the period 1907 March 12 to May 7 have been published by H. v. Zeipel (*A.N.* 4247, 1908), but do not include a minimum.

It is my pleasing duty to record my thanks to Professor Müller, at whose suggestion I took up the observation of this star, for the assistance that he has given me in the work.

Astrophysikalisches Observatorium, Potsdam:
1908 *June* 27.

Note on the Telegraphic Determination of the Longitude Greenwich—Ascension—Cape, in the Year 1908.
By Sir W. H. M. Christie, K.C.B., Astronomer Royal.

It was decided by the Hydrographer to take advantage of the visit of H.M.S. "Mutine" to Ascension Island in the early part of this year to re-determine the longitude of that place both from Greenwich and the Cape, and incidentally to obtain a new value for the Greenwich-Cape longitude. Arrangements were made for Captain Monro and Lieutenant Gibson of the "Mutine" to attend at the Royal Observatory, Greenwich, in the autumn of 1907 before leaving England, and also on their return in 1908, for the purpose of ascertaining their personal equation referred to the standard observer with the transit circle. Mr. Pett, of the Cape Observatory, who was in England in 1907, was also directed to attend at Greenwich for the same purpose. The arrangement for the work was left to me; and when the Eastern Telegraph Company, at the Hydrographer's request, kindly placed the use of their cable at our disposal, I arranged with Mr. Judd, Electrician-in-chief of the Company, and his assistant, Mr. Young, as to the method of signalling, etc., the essential points being that the syphon recorders of the Company at the several places should be used as chronographs, and that the signals should be transmitted without the interposition of relays. As the result of experiment, it was found impracticable to use one syphon only to record both the clock signals and the out-going or in-coming signals; and at each place, therefore, a second syphon was added to the recorders, making two parallel lines on the running tapes. At Ascension this second syphon was actuated by the chronometer, and at Cape Town and Porthcurno by the clocks at the Cape and Greenwich Observatories respectively. The Post Office authorities kindly granted the use of their wires between Greenwich and Porthcurno; and when signals were exchanged, these were joined up without relays, to make an unbroken line.

The circuit from the sidereal standard relay at Greenwich actuated a telegraphic sending apparatus, and a current was transmitted which recorded the seconds of the Greenwich clock on a tape chronograph at the Observatory, and also actuated the second syphon at Porthcurno, these two being synchronous except for the time of transmission from Greenwich to Porthcurno, which was only $0^{s}.006$. This quantity was found by sending return signals immediately after the exchange through the cable from Porthcurno to Greenwich, which were recorded on the syphon recorder and also on the chronograph at Greenwich. A very similar arrangement was made at the Cape, where the time of transmission was found inappreciable.

It is essential that if both the syphons of a recorder are actuated at the same instant, the deflections on the tape in the two lines should be exactly abreast one another; and if they are not, the distance between them must be ascertained and allowed

for. This difference of position between the syphons is called "parallax."

For the signals which were sent between Ascension and Porthcurno, the ends of the cables both at Madeira and St. Vincent, the two intermediate stations, were joined up. Similarly, when signals were sent between Ascension and Cape Town, the cables were joined at St. Helena, so that no intermediate relays were introduced, and signals were satisfactorily received through both these lengths of cable, distances of 4213 and 2687 nautical miles respectively.

The instrument used at Ascension for the determination of time was a reversible transit (D), one of the series of five originally made for the Transit of Venus expedition of 1874, and subsequently modified for use in various longitude determinations from 1888 to 1902. It was last used in the determination of the Greenwich-Paris longitude 1902, and had since then been fitted with the central illumination designed by Mr. Harold Christie and myself, and described in the *Monthly Notices of the R.A.S.*, 1908, January.

The Chronograph used for recording the times of the observations at Ascension, and also at Greenwich while ascertaining personal equation, was a small field chronograph made by Lindqvist. The tape is driven by clockwork, the speed of which can be regulated within certain limits. The clock and tapper records are made with needle points so fitted to the ends of levers that after striking the tape vertically they will rock against the push of a light spring in a direction parallel to the motion of the tape, and by this means the point is withdrawn from the paper without tearing it, though this, of course, would only occur in case of the lever not rising immediately after striking.

The Chronometer used at Ascension was a quick train chronometer by Pennington, and fitted by Lindqvist for use in connection with the field chronograph. An extra wheel is fitted on to the arbor of the seconds hand, on the outer edge of which are 60 teeth. An agate point on the spring contact is pressed forward by these teeth, and remains there during the period of one beat. As the wheel revolves, the agate point slips back into the notch following, and thus breaks the contact. The teeth of the wheel are cut so that one contact is made at each second except the 60^s, and an extra contact just after the 30^s.

Captain Monro proceeded to Greenwich in 1907 September, and was shortly followed by Lieutenant Gibson, for instruction in the use of the instrument under Mr. Hollis, and to ascertain personal equation, remaining there three or four weeks. Observations for personal equation, with the same instrument as was used at Ascension, were made by Captain Monro on 9 nights and by Lieutenant Gibson on 4 nights.

The "Mutine" arrived at Ascension on January 20, and a site was at once selected for the instrument. This was on the rising ground at New Krutown, between the ends of the two Northern

buildings. From the Observation Pillar at the settlement it bore S. 34° 17′ 43″ W. distant 1524 feet. This position was selected with a view to obtaining a clearer South and Zenith sky clear of the line of cloud which at times streams away to leeward from the summit of the island. A meridian mark was erected at a little over half a mile to the southward on the sky line.

Observations were begun on the night of January 26, and continued nightly by one or both observers until February 28, excepting only 3 nights when clouds prevented. Signals were exchanged with both the Cape and Porthcurno on the nights of February 1, 2, 8, 9, 16, 22, and 23, and on the 15th with the Cape only, the land line between Greenwich and Porthcurno having broken down on that night.

Captain C. H. Simpson, R.N., was appointed to superintend the signalling operations at Greenwich. He also read all the signal tapes from each of the stations. The signals for comparison of clocks were sent from Porthcurno, Ascension, and Cape Town by means of the ordinary apparatus used by the Cable Company for the transmission of messages, by hand, at intervals of 10 seconds for periods of about 3 minutes, giving approximately 20 signals to each set. The time of transmission between Porthcurno and Ascension was found to be $0^{s}\cdot529$, while that between Ascension and Cape Town was $0^{s}\cdot250$.

On the return of the "Mutine" to England in 1908 July, Captain Monro and Lieutenant Gibson made a further determination of personal equation at Greenwich, observations being obtained on 8 nights by each observer.

The programme of observations at Ascension on each night consisted generally of the observation of 16 clock stars by each observer, symmetrically arranged with respect to the two positions of the instrument, micrometer east and west, and a sufficient number of stars of greater declination both north and south for the determination of azimuth error, together with an adequate determination of level. The two observers at Ascension observed groups of stars alternately, and the order of micrometer east and west was usually reversed on alternate nights.

The methods of determining constants of the instruments and instrumental errors were essentially the same as those described in the Greenwich publication, "Determinations of Longitude, 1888–1902." The equatorial intervals of the vertical wires were determined first at Greenwich and afterwards at Ascension by means of the transits of a large number of clock stars, 100 in the first case, and 240 in the second, observed over all nine wires. The collimation error used in the reduction was determined by the observations of the nadir which were made at the beginning and end of each night's work, but this error is finally determined by the reversal of the instrument.

During the observations for personal equation at Greenwich in 1907 there was no sensible discordance between the clock errors found in the two positions of the instrument, but at Ascension and

at Greenwich in 1908 July a difference was found of about $0^{s}.25$ by both observers, and it would seem, therefore, that some change must have taken place in the instrument in transit from Greenwich to Ascension. Inequality of pivots might account for this difference; but if this existed, the level error determined in the two positions of the instrument would show a similar disagreement. The observations of level, however, made during the longitude operations, and also some special series made in 1908 September at Greenwich, show a difference in West – East of only about $+0''.35$ in the mean, which would cause a difference of clock error W. – E. not greater than $0^{s}.04$, and would not therefore account for the discordance observed. It seems therefore reasonable to treat the difference purely as an error in the determination of collimation. It is possible that it might be due to some error in the adjustment of the Bohnenberger eyepiece used in the determination of collimation by the nadir in reversed positions of the instrument. Experiments were made at Greenwich to test this, but the collimation errors determined by observation of a south mark and nadir by reversal in both cases were in agreement.

On the other hand, the polar stars observed at Greenwich in 1908 in reversed positions of the instrument agree with the clock stars in the collimation error which they indicate. In any case it may be taken that the error is eliminated by taking the mean for the two positions of the instrument.

Level Error was determined by the striding level and by observation of the nadir. In the determination with the striding level, the micrometer screw was used so as to eliminate as far as possible any differences of scale value in different parts of the tube. The difference of level error as ascertained by the striding level and nadir for the nights on which exchanges of signals were made amounts in the mean to $0^{s}.014$.

The Azimuth Error.—The value of "n" adopted each night was deduced from usually four observations with the micrometer E and a similar number micrometer W. The stars for this purpose used at Ascension were generally at some distance from both North and South poles, owing to the latitude of the place being small, but for this reason also the correction introduced by the azimuth error is exceedingly small, as the clock stars used were all near the zenith.

The adopted error of the Sidereal Standard clock at Greenwich is the weighted mean of errors, corrected for personal equation referred to the standard observer, found by the various observers with the Transit Circle, and the rates used are those deduced from these errors.

The adopted clock error at the Cape is that obtained by Mr. Pett using the non-reversible Transit Circle at the Cape, these errors and adopted rates having been supplied by H.M. Astronomer at that place.

The differences of longitude of Greenwich—Ascension and Cape—Ascension have been calculated, using the errors obtained by each observer at Ascension independently. Rates of the chronometer were calculated from the observations obtained by each observer, and

from these and from the rates obtained both from the Greenwich and Cape clocks by comparison of signals the adopted rates were deduced.

The following Table I. shows the difference of the error of the Sidereal Standard at Greenwich as found by Captain Monro and Lieutenant Gibson, and the corresponding adopted error found by the standard observer with the Greenwich Transit Circle, the errors by the longitude observers being reduced to the Greenwich meridian. Table II. shows the error found by Mr. Pett observing with the Greenwich Transit Circle, compared with the standard observer. Table III. gives the difference of longitude found by each observer on each night, the error of the chronometer at Ascension being compared with the adopted error of the Sidereal Standard at Greenwich as found by the standard observer and with the error of the Standard clock at the Cape Observatory as found by Mr. Pett, the personal equation in the preceding tables being applied.

TABLE I.

Table of Personal Equations of Captain Monro and Lieutenant Gibson, using Transit D, compared with the Standard Observer with the Transit Circle as found at Greenwich 1907 *and* 1908.

Captain Monro.

Date. 1907.	Personal Equation. Transit Circle with Annular Illumination.	Date. 1908.	Personal Equation. Transit Circle with Central Illumination.
	s		s
Sep. 18	+0·28	Jul. 14	+0·42
19	·16	20	·21
23	·26	22	·18
24	·33	23	·19
30	·42	24	·21
Oct. 2	·31	27	·22
4	·21	29	·25
7	·27	Aug. 7	·29
8	·35		+0·246
	+0·288	Correction to reduce to annular illumination.	+·056
			+0·302
	Adopted		+0s·295

Lieutenant Gibson.

Date. 1907.	Personal Equation. Transit Circle with Annular Illumination.	Date. 1908.	Personal Equation. Transit Circle with Central Illumination
	s		s
Oct. 2	+0·34	Jul. 20	+0·24
4	·19	21	·35
7	·20	22	·21
8	·32	23	·00
	+0·263	24	·01
		27	·22
		29	·24
		Aug. 7	·14
			+0·176
		Correction to reduce to annular illumination.	+·056
			+0·232
		Adopted	+0s·248

The central illumination of the Greenwich Transit Circle was fitted in 1908 April, after the observations at Ascension in connection with the longitude determination were completed, the annular illumination being in use at that time. The above correction (0s·056) calculated from 31 comparisons made with the two methods of illumination between April 30 and September 26 has therefore been applied.

TABLE II.

Table of Personal Equation of Mr. Pett, compared with the Standard Observer, both observing with the Transit Circle at Greenwich in 1907.

Date.	Personal Equation.	Date.	Personal Equation.
	s		s
Sept. 16	+0·09	Sept. 24	−0·02
17	+ ·01	26	− ·01
18	+ ·05	27	+ ·02
19	+ ·00	Oct. 2	+ ·03
20	+ ·01	4	+ ·06
23	− ·06	Adopted	+0·02

Note.—In the above tables a + sign means that the quantity is to be added to the "clock slow" found by the observer to find "clock slow" by the standard observer.

TABLE III.

Difference of Longitude: Greenwich—Ascension—Cape.

Final Results.

Date. 1908.	Difference of Longitude by M.	Divergence from Mean.	Difference of Longitude by G.	Divergence from Mean.	Time of Transmission.
	Greenwich—Ascension.		Greenwich—Ascension.		
	h m s	s	h m s	s	s
Feb. 1	0 57 40·492	+0·094	0 57 40·476	+0·122	+0·545
2	·355	− ·043	·285	− ·069	+ ·519
8	·249	− ·149	·239	− ·115	+ ·518
9	·367	− ·031	·275	− ·079	+ ·528
16	·462	+ ·064	·404	+ ·050	+ ·536
22	·496	+ ·098	·430	+ ·076	+ ·537
23	·368	− ·030	·372	+ ·018	+ ·519
	0 57 40·398	±0·073	0 57 40·354	±0·076	+0·529
	Cape—Ascension.		Cape—Ascension.		
	h m s	s	h m s	s	s
Feb. 1	2 11 34·994	−0·039	2 11 34·978	−0·006	+0·251
2	·998	− ·035	·928	− ·056	+ ·258
8	35·035	+ ·002	35·025	+ ·041	+ ·256
9	·032	− ·001	34·940	− ·044	+ ·252
15	·071	+ ·038	·993	+ ·009	+ ·236
16	·082	+ ·049	35·024	+ ·040	+ ·251
16	·066	+ ·033	·008	+ ·024	+ ·249
22	·030	− ·003	34·964	− ·020	+ ·253
23	34·993	− ·040	·997	+ ·013	+ ·246
	2 11 35·033	±0·027	2 11 34·984	±0·028	+0·250

By subtracting the values of the longitude determined night by night (Cape—Ascension)—(Greenwich—Ascension) values are obtained of the difference of longitude between Greenwich and the Cape independently of the error of the chronometer found at Ascension. The following is a series of values so found from the above results. For Feb. 16 the result from the first exchange of signals Cape—Ascension has alone been used, as that exchange more closely followed the exchange between Greenwich and Ascension.

Greenwich—Cape.

	h m s	s
Feb. 1	1 13 54·502	−0·123
2	·643	+ ·018
8	·786	+ ·161
9	·665	+ ·040
16	·620	− ·005
22	·534	− ·091
23	·625	·000
	1 13 54·625	±0·063

From the foregoing it will be seen that the results, so far as they can be at present determined, and adopting the mean of the two observers at Ascension, are as follows:—

	h m s	s
Greenwich—Ascension	0 57 40·37	±0·025
Cape—Ascension	2 11 34·99	±0·008
Greenwich—Cape	1 13 54·62	±0·022

The probable errors here given are deduced solely from the divergences in these last three tables.

The longitude of the Cape hitherto adopted is $1^h\ 13^m\ 54^s{\cdot}76$ E, from which the above result differs but little.

Royal Observatory, Greenwich:
1908 *November* 9.